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## The political economy of immigrant legalisation: evidence from the 1986 IRCA

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# The political economy of immigrant legalisation: evidence from the 1986 IRCA

## Abstract

What happens to the distribution of public resources when undocumented migrants obtain legal status through nation-wide amnesty? In this paper, we exploit variation in legal status from the 1986 Immigration Reform and Control Act (IRCA) to answer this question and find that state governors, of whatever party affiliation, allocate more per capita aid to those counties affected by the IRCA. We posit that this is borne out of rational, forward-looking governors who allocate resources strategically in pursuit of the votes of the newly legalised who were eligible to vote five years after legalisation. To support this view, we find that the distribution of state aid differs significantly according to political context. Counties affected by the IRCA receive more resources from the state when their governor is eligible for re-election, faces political competition or enjoys line-item veto power. Our results also indicate that the transfers were targeted to the newly legalised, who by and large were of Hispanic origin, and not other constituents. We find no evidence of anti-migrant sentiment confounding our results. Counties that received more transfers from the governor also experienced improvements in Hispanic high school completion rates.

JEL-Codes: J150, H720, P160.

Keywords: immigrant legalisation, distributive politics, state and local government.

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## 1. Introduction

Undocumented migration has today become a hotly contested issue. This contest is, perhaps, fiercer nowhere than in the United States where, as of 2016, an estimated 11.3 million migrants reside in the country illegally, up from 5.8 million in 1996.<sup>1</sup> Given the polarised nature of the issue, we ask in this paper what happens to the distribution of state and local finances when undocumented migrants are offered legal status through a nation-wide amnesty. We answer the question by focussing on the actions of incumbent state governors: how do they respond, in terms of public resource allocation, to the incentives created by documenting undocumented migrants? Do they allocate more public resources to the areas affected by amnesty and to what extent is such allocation intended to capture the votes of the newly documented migrants as opposed to other, perhaps competing, constituents?

The history of the United States offers a unique opportunity to study these issues. In 1986, the Reagan Administration passed into law the Immigration Reform and Control Act (IRCA) which legalised nearly 3 million undocumented migrants in the span of some three years and offered them a path to citizenship five years after legalisation. In this study, we combine variation in legal status from the IRCA with administrative data from the Census of Governments in order to throw light on the ways in which documenting undocumented migrants affects the distribution of state and local finances. Our analysis is motivated by a model of distributive politics in which incumbent politicians react to changes in the electorate by adjusting their budget allocations to target the preferences of the newly enfranchised group so as to optimise (a) the welfare of the population and (b) their own re-election chances. Accordingly, we posit that governors, who play an important role in formulating the state budget, will allocate resources strategically to those counties most affected by the IRCA in an effort to win the political support of the newly documented migrants who were eligible to vote five years after legalisation.

Using a difference-in-differences regression framework, we compare the distribution of public finances—specifically, per capita inter-governmental transfers from state to local governments—in counties that experienced more per capita legalisations with those that received less both before and after 1986. Our baseline estimate suggests that counties with a greater share of IRCA-documented migrants received more per capita transfers from their state governments than those with a fewer share of such migrants. The result is not driven by differences in county social, economic or demographic characteristics and is robust to alternative specifications and samples.

To overcome potential geographical endogeneity associated with where undocumented migrants settle, we follow two approaches: first, we employ propensity score matching to identify a more comparable control group and second, we use the share of a county's 1960 population that is foreign-born as an instrument for the number of documented migrants per county post-1986. These tests confirm that the baseline result is not confounded by geographical

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<sup>1</sup>Pew Research Centre, taken from <http://pewrsr.ch/2oWIM93> Accessed 9 February 2018.

factors.

Arguably, our result may be driven by mechanical or bureaucratic forces that oblige the state governor to better service the areas where the newly documented migrants reside rather than, as we posit, discretionary forces borne out of political calculation. To refute this competing explanation we analyse, in the second part, the sensitivity of inter-governmental transfers to political constraints. The rationale is straightforward. If the transfers of the state government are reflective of forces outside the control of the governor, then the result ought to be insensitive to political constraints faced by the governor. If, on the other hand, the transfers are politically motivated, the result ought to exhibit heterogeneity with respect to the various political constraints facing the incumbent. Consistent with this line of thinking, we find that counties affected by the IRCA receive more resources from the state when their governor is eligible for re-election, faces political competition, enjoys line-item veto power over the budget or is politically aligned with the state legislature. We also uncover heterogeneity along party lines: although both Republican and Democratic governors allocate significantly more resources to IRCA-affected counties, the effect increases by about half when the governor is a Democrat. It is, perhaps, unsurprising that we also find that a governor's likelihood for re-election increases in the share of newly documented migrants in a state.

Finally, the IRCA provided a path to citizenship five years after legalisation. How plausible is it, then, that governors target their transfers to actually meet the needs, and win over the future political support, of the newly legalised as opposed to other, perhaps competing, constituents in a county? To address this issue, we first undertake a number of empirical checks to alleviate concerns that anti-migrant sentiment may drive our results. In this respect, we exploit data on a prominent anti-migrant ballot measure in California and find no relationship between state transfers and the interaction between a county's share of legalised migrants and its support for the ballot measure. More generally, we utilise survey data from the entire United States and find that, if anything, areas more affected by the IRCA actually experience improvements in attitudes towards both documented and undocumented migrants, further suggesting that anti-migrant sentiment is not confounding our results. Next, we turn our attention to local expenditure and find that expenditure in education increases in the share of newly documented migrants in a county. Hispanic individuals, as opposed to Caucasian ones, residing in these counties and who entered middle school after 1986 experience improvements in the likelihood of completing high school, suggesting that the additional resources a county receives on account of the IRCA were in fact targeted to the newly legalised as opposed to other voting blocs.

Together, these results point to a strong, nuanced political economy facet of immigrant legalisation. Although models of distributive politics generally predict that the expansion of franchise leads to greater resources being allocated to those who have a new-found political voice, it is more of an open question to what extent politicians allocate resources to capture this new "swing" vote as opposed to further solidify a core constituency. Our results indicate that, at least in the context of undocumented migration, politicians allocate resources primarily in an

effort to capture the political support of the new swing vote rather than that of other groups.

Our paper offers two main contributions. First, we contribute to the literature that sheds light on the distributional effects of the expansion of voter franchise. Cascio and Washington (2014), for example, study enfranchisement of African Americans in the United States through the Voting Rights Act of 1965. They find that counties that removed literacy tests at voter registration in response to the law experienced greater voter turnout among black voters which, in turn, increased the share of public spending directed towards them. In a similar vein, Miller (2008) shows that the enfranchisement of women in the United States was followed by immediate changes in legislative behaviour and substantial increases in public health spending at the local level. Examining the impact of electronic voting technology in Brasil, Fujiwara (2015) finds that the enfranchisement of lesser educated citizens affected government spending and increased health care spending, both of which are particularly beneficial for lower-income people. Naidu (2012), looking at a case of disenfranchisement, analyses the effects of the introduction of poll taxes and literacy tests in the 19<sup>th</sup> century United States and finds that such measures lowered overall voter turnout with the subsequent effect of worse educational outcomes for black pupils and losses in annual income in the order of 15 percent for black labourers. We extend this literature to consider the case of undocumented Hispanic migrants in the United States, one of the largest disenfranchised groups in the country. Moreover, because of our rich data on state governors, we are able to examine some of the political mechanisms that lead governors to allocate resources in light of a sudden shock to the electorate.

Second, we contribute to the literature on the economics of legal status. In this respect, the IRCA has been used as a credible policy shock to identify the impact of legal status on various social and economic outcomes at the level of the individual migrant.<sup>2</sup> For example, Cortes (2013) shows that legal status helps migrants to obtain better educational outcomes whilst Kossoudji and Cobb-Clark (2002) and Pan (2012) use the IRCA to show that documenting undocumented migrants leads to an improvement in their wages, employment prospects and ability to speak English. We contribute to this literature by examining the influence of legal status directly on the distribution of public resources at the state and local level, bringing to light a dimension of undocumented migration that is highly debated yet relatively understudied.

The rest of this paper proceeds as follows: Section 2 contextualises the study by discussing the historical background of the IRCA as well as the demographic characteristics of its applicants. In Section 3 we present a simple framework that guides our empirical analysis and the interpretation of our findings. Section 4 describes our data and explains institutional features associated with the budget-making process at the state level. Section 5 outlines our econometric methodology and, along with sections 6 and 7, presents our results. Section 8 concludes.

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<sup>2</sup>Although some studies consider the impact of legal status on more aggregate outcomes. Baker (2015), for example, finds that counties with greater shares of IRCA applicants experienced a decline in crime rates.

## 2. Background

### 2.1 The Immigration Reform and Control Act

The Immigration Reform and Control Act (IRCA) of 1986 was, to date, the most extensive piece of legislation put forward by the United States government to address the question of undocumented immigration. The passage of the IRCA was by no means straightforward. It began in the 1970s when the legislative and executive branches of government considered various elements of comprehensive immigration reform. These efforts gained momentum when, in 1977, Congress appointed the Select Commission on Immigration and Refugee Policy which presented, in 1981, a proposal for immigration reform which was ultimately rejected. In the years that followed, several other proposals were put forward and variants of the IRCA were passed through either the Senate or the House but none was able to win complete approval until the 99<sup>th</sup> Congress settled on and approved the IRCA on 17 October 1986 and which was signed into law on 6 November 1986.<sup>3</sup>

The purpose of the IRCA was to restrict the flow of undocumented migrants into the United States. It rested on three main pillars: an employer sanctions provision that made it illegal for employers to knowingly hire unauthorized workers; increased funding for border security to discourage new people from migrating illegally; and an amnesty programme intended to legalise various unauthorized workers (Chishti and Kamasaki, 2014).

While each of the components of the law was not without problems (in particular the employer sanction scheme which led to a great amount of fraud as well as workplace discrimination), the legalisation programme is generally regarded as the law's most successful provision. It provided two programmes for two distinct groups of unauthorized workers. First, the Legally Authorized Workers (LAWs, also known as "pre-82s") under section 245A of the law enabled undocumented immigrants who resided in the country for an uninterrupted period from before 1 January 1982 to legalise (DHHS (December 1991), Cascio and Lewis (2018)). Second, the Special Agricultural Workers (SAW) under Section 210 of the law allowed applications from unauthorized migrants who could show that they carried out 90 days of work on select USDA defined seasonal crops in the year leading to 1 May 1986 (DHHS (December 1991); Cascio and Lewis (2018)). LAW applicants were eligible to apply within a 12-month time frame extending from May 1987 to May 1988 whereas SAW applicants had an 18-month application period from 1 June 1987 to 30 November 1988 (DHHS, December 1991). On acceptance of their application, applicants were given temporary legal status under the title of *Temporary Resident Aliens* which could last for as long as 18 months. After this period, and upon successful completion

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<sup>3</sup>The timing of the IRCA's passage in 1986 was indeed sudden and unexpected. Just days before its passage in Congress, "congressional leaders pronounced it dead, this time after more than fifteen months of hearings, legislative negotiations and debate" (Fuchs, 1990). Speaking to this idea, Representative Daniel E. Lungren (R-California) remarked on the day of the bill's passage that the IRCA was "a corpse going to the morgue, and on the way to the morgue a toe began to twitch and we started CPR again" (Fuchs, 1990). See Table A.1 for details on how Congress voted to pass the bill.



of an English test and a civics test, applicants were given permanent resident status.

At the time of the Act, there were some 3 million undocumented immigrants residing in the United States, corresponding to nearly 1 percent of the population (Wasem, 2012). The law stipulated that both application periods (the 12 months for the LAW programme and 18 months for the SAW programme) were strictly enforced, which from an econometric point of view implies a relatively clean identification period. Indeed, by the end of the application period, roughly 3 million people applied for temporary resident status, of which 1.7 million comprised LAWs and 1.3 million comprised SAWs (DHHS, December 1991). By 1990, 94.6 percent and 58.7 percent, respectively, of LAW and SAW applications had received approval and for all intents and purposes, the legalisation programme of the IRCA ceased to generate newly legalised citizens after 1990 (DHHS, December 1991). Figure 1 shows the time trend of the stock of IRCA legalised migrants while Figure 2 shows the geographic distribution of IRCA applicants at the county level in 1992 for those counties for which data is available.<sup>4</sup> As shown, the majority of legalisations took place between 1986 and 1990 and in the states of California (970,895), Texas (351,646), Illinois (125,399), Arizona (70,488) and New Jersey (29,012). As shown in Figure 3, undocumented migrants applied for legal status in approximately 330 counties whereas the remaining counties received no such applications.

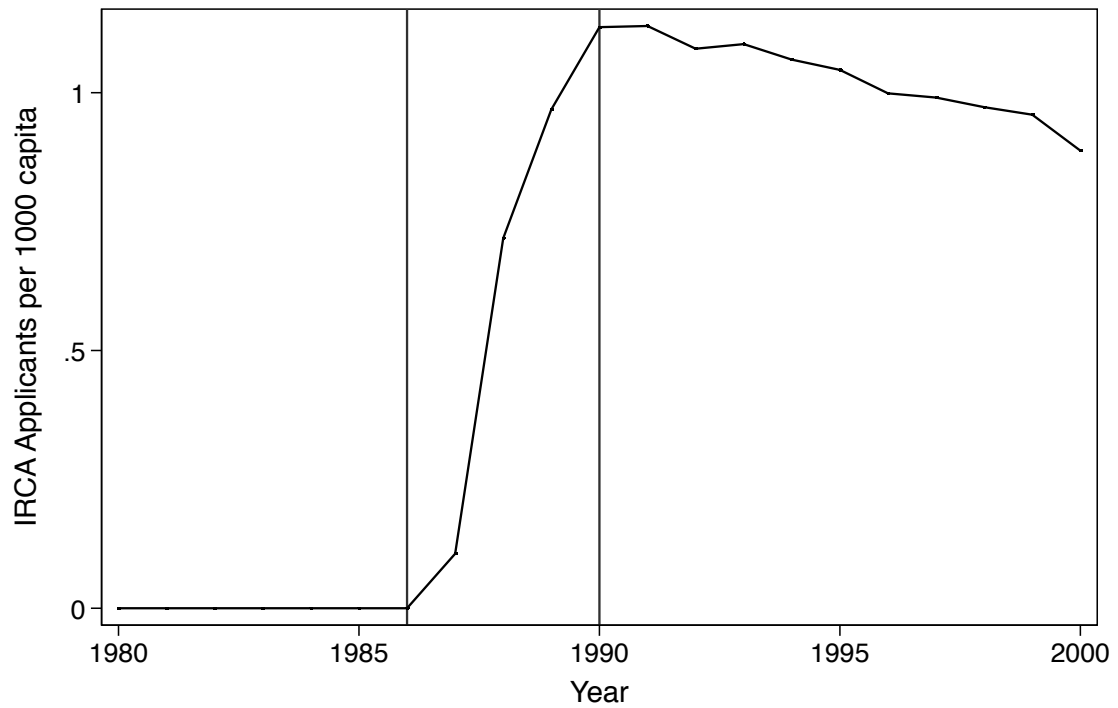
## 2.2 Demographic Characteristics of the Legalised

Figure 4 presents data from the December 1991 report to Congress from the Department of Health and Human Services which documented the characteristics of the legalised population. These data indicate that the newly legalised are predominantly of working age, healthy and with relatively few children. More than half and two-thirds, respectively, are single and male and the vast majority of applicants were engaged in full-time work. Fully 22 percent of all applicants reported a household income of over \$600 per week; well over the poverty line, which, in 1989 stood at \$6,311 for a single person (\$121 per week) and \$12,675 for a family of four (\$244 per week) (Mosbacher and Bryant, 1991).<sup>5</sup> In fact, median take-home pay for IRCA applicants stood at \$400 per week. Median household income in the population in 1989 stood at \$23,745, or \$456 per week. The report also makes clear that no more than 5 percent of the migrants reported not being able to work in the month prior to the survey. As such, IRCA applicants were, by and large, an economically active and self-reliant group earning somewhere between the poverty threshold and median income.

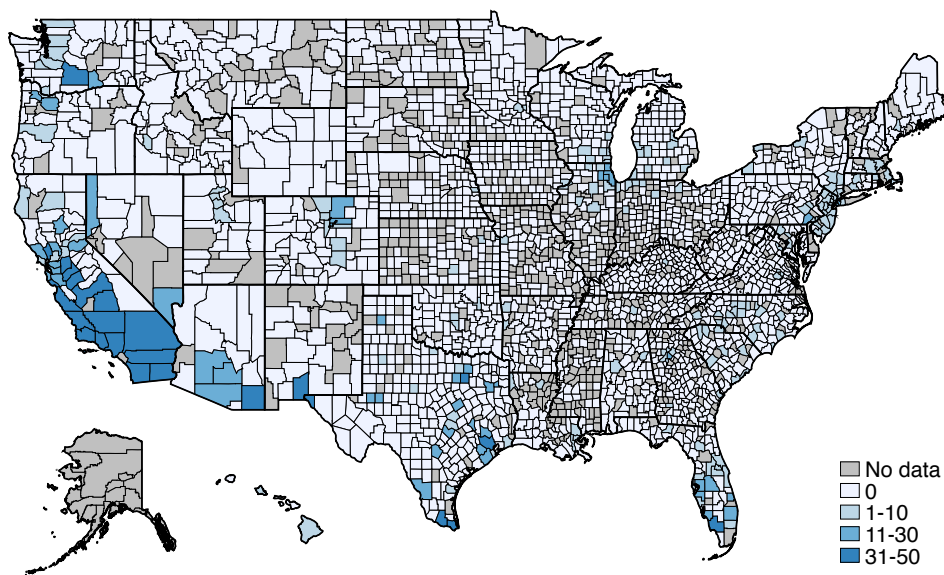
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<sup>4</sup>Of the 3,142 counties in the United States, our dataset includes IRCA information on 2,760 of them (and from all states except Alaska and Delaware). However, we do not observe every county in every year because some of the counties drop out in the later stages of the sample. As such, the actual number of treated counties varies slightly in the sample from 276 counties in 1999 to 332 in 1991/92. Restricting the analysis to only those counties that we observe throughout the entirety of the sample makes no difference to the results.

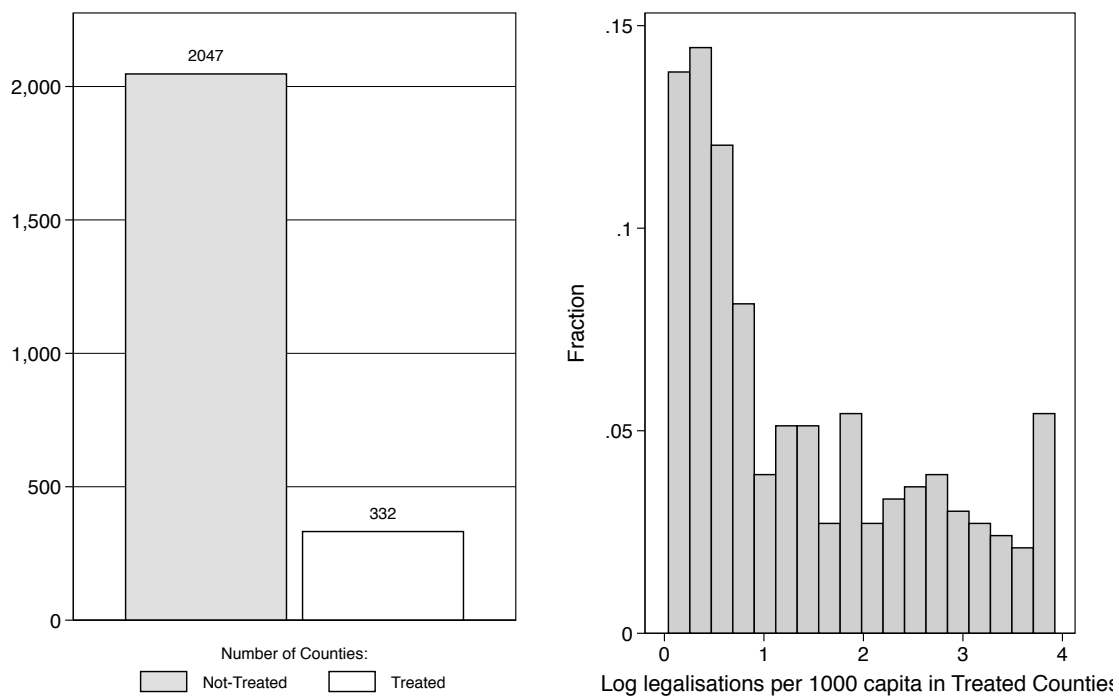
<sup>5</sup>The National Longitudinal Survey of the U.S. Bureau of Labour Statistics suggests the poverty thresholds in 1989 were even lower: \$5,980 for a single person and \$12,100 for a family of four. Taken from <http://bit.ly/2tGnz8V>, accessed in August 2017.



**Figure 1:** Stock of IRCA applicants at the county level per 1,000 capita

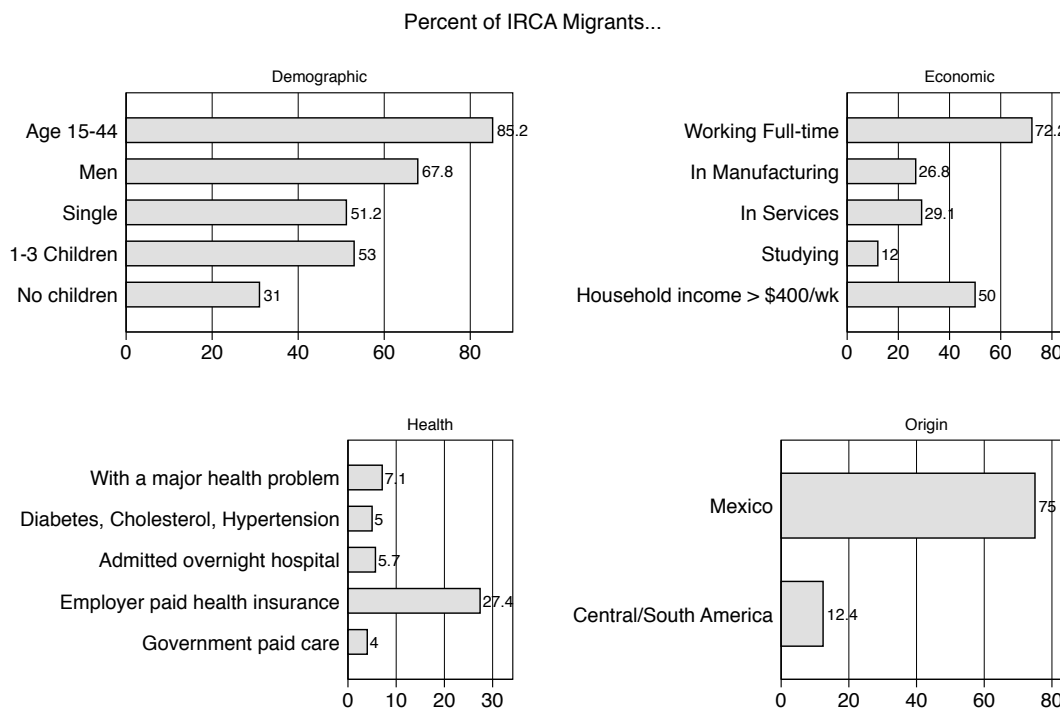


**Figure 2:** Number of legalized migrants per 1,000 county inhabitants in 1992



**Figure 3:** Distribution of legalised migrants

**Note:** This graph plots the distribution of the legalised migrants in 1992. The left panel indicates the number of treated (332) and control (2,047) counties whereas the panel on the right plots the distribution of treatment within treated counties. On a linear scale, the average treated county experienced 4.4 applications per 1,000 county inhabitants. The average population size of treated counties is just over 409,490, implying that on average 1,800 migrants received legal status in treated counties.



**Figure 4:** Socio-economic characteristics of the IRCA applicants

**Notes:** These are the characteristics of the IRCA migrants as reported by Congress in 1991.

**Source:** DHHS (December 1991)

### 3. Incumbent Politicians: A Framework

In this section, we present a simple framework to guide our empirical analysis. In this framework, an incumbent politician controls the distribution of transfers flowing from the state budget to the various counties in the state. The politician is concerned both with the welfare of the population and her own re-election. A sudden change of legal status in a large and homogeneous group of residents in a county will thus change the politician's decision on how to distribute state resources so as to optimize her re-election chances and the welfare of the population. In our setting, therefore, we expect that the legalisation of a large group of mostly Hispanic people of lower socio-economic status will prompt politicians to offer policy over public resources so as to target the preferred outcome of the newly legalised group in the hope of winning their future political support.<sup>6</sup>

<sup>6</sup>In theory, there are two channels through which a policy shock such as the IRCA can affect the distribution of public resources (Persson and Tabellini, 2000). On the one hand, it might prompt a distribution of resources that is broad and general, for example, social benefits to all the members of some broadly defined group, such as the unemployed or the elderly. On the other, such programmes may be targeted and specific, aiming to benefit a more narrowly defined subgroup of the population. Broad redistributive programmes are those that appeal to the majority of the electorate and reflect their policy preferences. More targeted programmes, by contrast, impose little cost on the majority of the electorate but offer great incentives for its beneficiaries. Although state tax revenue does increase with the share of documented migrants in a state, the tax rate does not increase as a function of IRCA applicants and we show results later in the paper that suggest the transfers IRCA-affected counties receive from the

### 3.1 Politician Pay-off

We begin by modelling the objective function of an incumbent politician,  $P$ . In doing so, we adapt a model presented by Englmaier and Stowasser (2017) to consider a case of a state governor. For every county in the state,  $P$  transfers an amount of government assistance,  $g > 0$ , to the population at cost  $C(g)$ . As mentioned above,  $P$  is concerned with the utility of the residents,  $U$ , in every county as well as the utility derived from her expected vote share in elections,  $\Omega$ .

Each county is composed of two types of inhabitants: already legal citizens,  $C$ , and newly legalised migrants,  $L$ . The population in each county is normalised to one such that the share of the population that is newly legalised is  $\alpha$  and the share of already legal citizens is  $1 - \alpha$ . Only citizens can vote and voting decisions are based on local economic conditions, such as government expenditure on various public services. Voters base their voting decisions not just on local conditions immediately prior to the election but throughout  $P$ 's term.

For every county in the state,  $P$  has a pay-off,  $\Pi$ , that is composed of three parts, shown in equation (1):

$$\Pi = \underbrace{(1 - \alpha)U_C(g) + \alpha U_L(g)}_1 + \underbrace{E \cdot \Omega[\phi((1 - \alpha)U_C(g) + \alpha U_L(g))]}_2 - \underbrace{C(g)}_3 \quad (1)$$

The politician is concerned with the well being of the population in each county. This is reflected in the first term of  $P$ 's pay-off,  $(1 - \alpha)U_C(g) + \alpha U_L(g)$ . We assume utility functions are concave such that  $U'_i(g) > 0$  and  $U''_i(g) < 0 \forall i \in \{C, L\}$ . The only way in which  $P$  can improve the utility of the population is through her allocation of  $g$ . Because the characteristics of the documented migrants presented earlier, we take it as given that  $U'_L(g) > U'_C(g) \forall g$ .

We assume there are gains to staying in office. Accordingly, the second term,  $E \cdot \Omega[\phi((1 - \alpha)U_C(g) + \alpha U_L(g))]$ , captures the pay-off  $P$  obtains from re-election.  $E$  is a binary variable that is one when  $P$  is eligible for re-election and zero when  $P$  is a lame duck. In every election for which  $P$  is eligible to run,  $\phi$  captures her expected vote share in that election which is a function of the well being of the population. We assume that  $\phi$  is a linear function bounded between zero and one. The utility  $P$  derives from this expected vote share is captured by  $\Omega$ . It is assumed that  $\Omega(\phi)$ , a strictly increasing, non-linear function with a negative third order derivative. Figure A.1 provides an illustration of what such a function might look like. As shown, the marginal utility derived from the expected vote share is the greatest at the inflection point of  $\Omega(\phi = \phi_T)$  which represents the winning threshold.

Finally, the last term of equation 1 indicates the costs,  $C(g)$ , to the incumbent associated with allocating  $g$  to a given county. These capture the opportunity costs associated with distributing  $g$  among the different counties in a given state so as to remain within the budget

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state are, in fact, used for targeted local expenditures.

constraint. Costs are sufficiently convex such that  $\frac{\partial^2 \Pi_P}{\partial g^2} < 0$ .

A rational incumbent politician thus maximises her expected pay-off over the allocation of state grants. That allocation is strongly affected by the share of newly legalised migrants,  $\alpha$ , in a county. From this follows Predictions 1 to 3 which help guide our empirical analysis.

**Prediction 1:** The optimal allocation of state aid increases in the share of newly documented migrants in a county.

**Prediction 2:** The optimal allocation of state aid is larger when  $P$  is eligible for re-election and less when (s)he is a lame duck.

**Prediction 3:** The optimal allocation of state aid becomes larger the closer the  $P$ 's expected vote share is to the winning threshold.

Proofs can be found in the Appendix.

## 4. Data and Institutional Context

### 4.1 Data

The key explanatory variable in our study is a measure of the number of IRCA applicants per 1,000 county inhabitants in the United States for the period between 1980 and 2000. In the treated counties (i.e. those counties that received at least 1 application for legal status), this value ranged from as little as .04 to as many as 50 applications per 1,000 county inhabitants.<sup>7</sup> To carry out our analysis, we compiled a new dataset from a number of different administrative sources. Table 1 shows summary statistics of the main variables in our study according to whether they are in treated or non-treated counties.

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<sup>7</sup>By 1992, treated counties received, on average, eight applications per 1,000 county inhabitants which translates into some 2,800 applications for legal status per treated county.

**Table 1:** Balance Table: Treated v. Untreated Counties in 1984

	Treated			Untreated			Difference	
	Mean	S.D	Counties	Mean	S.D	Counties	Mean	S.E
<i>County Characteristics:</i>								
Transfers (per capita USD1999)	144.0	[150.4]	307	147.4	[344.9]	1886	-3.37	(20.0)
Log of Transfers (per capita USD1999)	4.33	[1.31]	307	4.30	[1.31]	1886	0.027	(0.081)
Unemployment Rate	7.98	[1.84]	328	7.98	[2.27]	1892	-0.0017	(0.13)
Poverty Rate	11.9	[5.85]	328	16.8	[7.48]	1892	-4.94***	(0.43)
Population (1000)	377.3	[607.6]	307	31.7	[29.8]	1886	345.5***	(14.1)
Log of County Income	9.53	[0.19]	328	9.29	[0.18]	1892	0.24***	(0.011)
County Tax Revenue (Pc)	133.9	[116.1]	307	129.1	[130.3]	1838	4.81	(7.91)
Log of Total County Crimes (Pc)	-3.36	[0.64]	328	-3.67	[0.73]	1892	0.31***	(0.043)
1960 Population Foreign Born (%)	4.62	[3.79]	328	1.77	[2.21]	1892	2.85***	(0.15)
<i>Governor Characteristics:</i>								
Lame-Duck Governor	0.32	[0.47]	328	0.39	[0.49]	1892	-0.071**	(0.029)
State Has Term Limits	0.50	[0.50]	328	0.56	[0.50]	1892	-0.065**	(0.030)
Share Democratic Governor	0.66	[0.48]	328	0.78	[0.41]	1892	-0.13***	(0.025)
Governor Re-elected	0.43	[0.50]	30	0.65	[0.48]	209	-0.21**	(0.094)
Percent Votes Cast For D-President	38.2	[9.62]	328	36.7	[10.3]	1892	1.52**	(0.61)

Our measure of IRCA applications per county comes from Baker (2015) who, in turn, takes it from the Immigration and Naturalisation Service (INS). We also take from Baker (2015) measures of county poverty, population, unemployment and income, all of which are used as control variables in our analysis.

We aim at understanding the impact of the IRCA on the distribution of state and local finances and the sensitivity of this impact to political constraints. We thus add data on state and local finances taken from the US Census of Governments and use per capita inter-governmental revenues from state governments to local governments (counties, cities, municipalities) aggregated to the county as our dependent variable. We also utilise a host of governor related data including party affiliation of the governor, his or her name, an indicator for whether or not a governor is a lame duck and an indicator for whether (s)he enjoys line-item veto power in order to better understand the responsiveness of state politicians to the IRCA. These data are obtained from the Codebook for State Elections. We apply an instrumental variables strategy to confirm our OLS estimates, using the share of a county's 1960 population that is foreign-born as an instrument for the number of documented migrants per county post-1986. This variable is taken from the County and City Data Book prepared by the US Department of Commerce and the Census Bureau and made available by ICSPP under Study No. 7736.

#### 4.2 Inter-governmental Revenue and The Budget-Making Process

##### *Inter-governmental revenue (IGR)*

The primary dependent variable is per capita inter-governmental revenue (IGR) received by local governments (counties, cities, municipalities, aggregated to the county) from state governments.<sup>8</sup> The Census Government Finance and Employment Classification Manual defines this variable as “[a]mounts received directly from the state government, including federal aid passed through the state government and state aid channelled through intermediate local government (e.g counties) which have no discretion as to its distribution. [It] includes state grants-in-aid, regardless of basis of distribution.” Correspondence with staff at the Census Bureau confirms that “each state determines what specific funding sources (if any) are used for grants to local governments.” and that “each state determines the nature, amount and distribution of state grants internally.”<sup>9</sup>

Local governments have few major sources of local revenue, most notably property taxes and, to a much lesser extent, sales taxes. Inter-governmental revenue, therefore, is a type of budget support for local governments which comprises, on average, 30 percent of all local government revenue as shown in Figure A.2. Importantly, the local governments that receive this aid have “no discretion as to its distribution”. State and federal governments decide to what areas these revenues are directed. For example, state aid in education is intended to “support of

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<sup>8</sup>We use inter-governmental revenue, state aid and state transfers interchangeably.

<sup>9</sup>Personal correspondence with Michael Fredericks of the Local Government Finance Statistics Branch of the Census Bureau on 26 November 2018.



local schools” but excludes “State grants for libraries”. The variable at our disposal is aggregate inter-governmental revenue at the county level and so it lumps together revenue intended for such areas as health, highways, education and public welfare.<sup>10</sup>

### *The budget-making process*

Our main contention is that state governors use their budgetary powers to allocate more resources to newly documented migrants in the hope of winning their future vote. A crucial question is thus how much power governors actually exert over the budget-making process. We take up this question in this section and demonstrate that, in fact, governors have substantial influence in the formulation and implementation of the states fiscal priorities.<sup>11</sup>

For the vast majority of states, the budget-making process takes an entire year: it begins sometime in July or August and for all but four states, the fiscal year begins on 1 July.<sup>12</sup> The state budget office is responsible for the analysis and preparation of the budget on behalf of the governor.<sup>13</sup> The budget-making process begins when the state budget office requests proposals from, and provides guidance to, various state-level agencies. This guidance typically includes state spending targets, assumptions for inflation and priorities of the governor. In the fall, the various agencies submit their budget proposals to the governor who reviews them and provides additional direction. Once the governor’s recommendations are incorporated, he or she presents the proposed budget to the state legislature in the winter season. After the legislature passes the budget, it requires the governors signature to become law.

Importantly, governors enjoy a number of powers over the budget-making process, including being able to spend unanticipated funds without legislative approval or to withhold appropriations from agencies within the executive, legislative or even judicial branches of government. Crucially, governors enjoy various forms of veto authority over the state budget. Depending on the state, governors have the authority to either veto the entire budget or specific line-items of it, a power which gives them great leverage over the prioritisation of the budget. Later in this paper, we document heterogeneity in our results depending on the extent of veto power a governor enjoys.

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<sup>10</sup>Although we only observe aggregate revenue, Table A.2 in the Appendix details what is and what is not included in the inter-governmental revenue received from the state and gives an indication as to what types of local activity these revenues support.

<sup>11</sup>The information in this section draws from the National Association of State Budget Officers report on the budget-making process NASBO (2015).

<sup>12</sup>In theory, 30 states operate an annual budget cycle and 20 operate a biennial budget cycle. In practice, however, most states employ a combination of both: in those states that operate a yearly budget, it is not uncommon for the governor to release spending recommendations for a two-year time horizon. States on a two-yearly budget cycle, by contrast, often prepare a supplemental budget which, in many cases, acts as a de facto yearly budget.

<sup>13</sup>The budget director is appointed directly by the governor in 34 states; in 13 states he or she is appointed by the department head and in one state the governor and department head share responsibility for this appointment.

## 5. Immigrant Legalisation and Inter-Governmental Revenue

### 5.1 The Evolution of IGR: Raw Data

Our aim is to understand the impact of documenting undocumented migrants on the distribution of inter-governmental transfers from state to local governments. The primary identifying assumption of our econometric model is that no other shocks occurred around the same time as the passage of the IRCA that correlate either with the number of legalised migrants in a given county or with the amount of inter-governmental revenue it received from the state. Prior to estimating the parameters of the model, therefore, it is informative to understand the evolution of IGR over time so as to lend credence to our identifying assumption. Figure 5 shows the trends in IGR as it appears in the raw data for the period between 1980 to 2000 in those counties that received applications for legal status with those that did not. As shown, the two county types developed along similar paths prior to the passage of the IRCA in 1986 and only after the passage of the law does one observe an appreciable difference between the two.<sup>14</sup>

As a more rigorous test for pre-treatment differences, we plot the coefficients of an event study as specified in equation 2:

$$y_{c,t} = \delta_c + \sum_{j=1980}^{2000} \beta_j [T_c \times D_t^j] + \epsilon_{c,t} \quad (2)$$

Where  $y_{c,t}$  is per capita inter-governmental revenue from state to local governments (in 1999 USD) in county  $c$  in year  $t$ ;  $T_c$  is a binary variable set to one if a county received one or more applications for legal status post-1986 and zero otherwise; and  $D_t^j$  is a dummy set to one when  $t = j$  ( $\forall j \neq 1986$ ). County fixed effects are captured by  $\delta_c$  and  $\epsilon_{c,t}$  is an idiosyncratic disturbance term clustered at the county level. The results are shown in Figure 6, which indicate that the difference in transfers received between treated and non-treated counties shown in Figure 5 only becomes positive and significantly different to zero in the years after 1986, further increasing confidence in the reasonability of our identifying assumption.

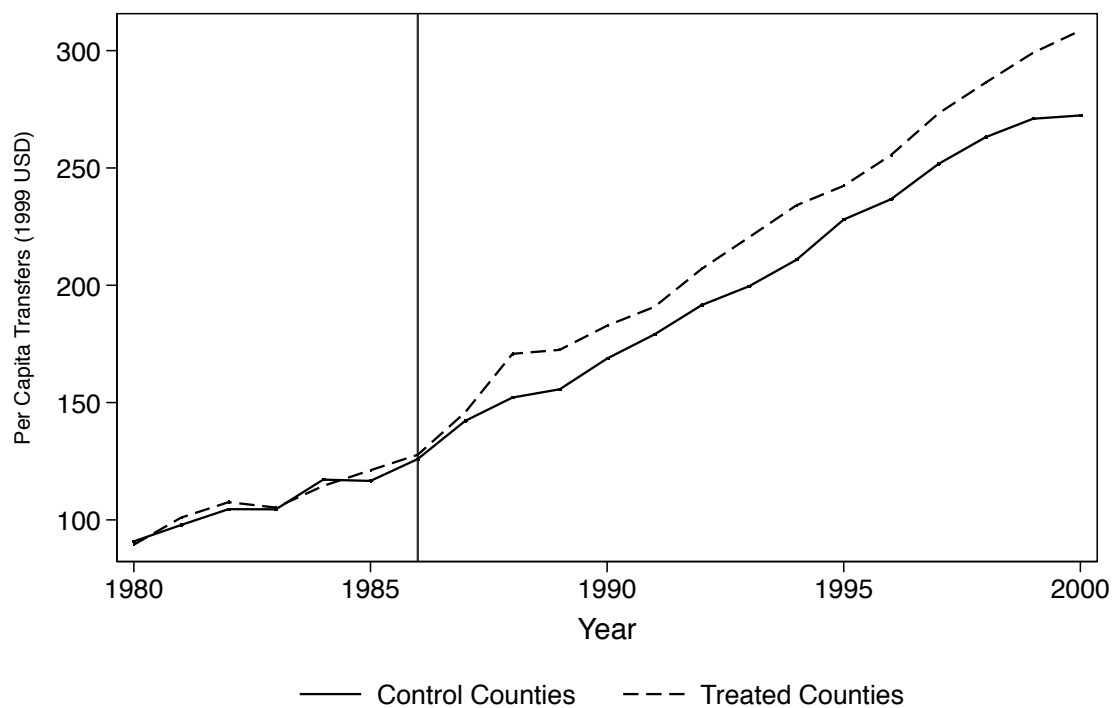
### 5.2 Baseline Estimates

We impose more structure on model 2 in order to estimate the parameters of a difference-in-differences regression specification as detailed in equation 3.

$$\ln(y)_{c,t} = \delta_c + \zeta_{st} + \beta_1 \cdot (T_c \times P_t) + \Theta \cdot \mathbf{X}_{c,t} + \epsilon_{c,t} \quad (3)$$

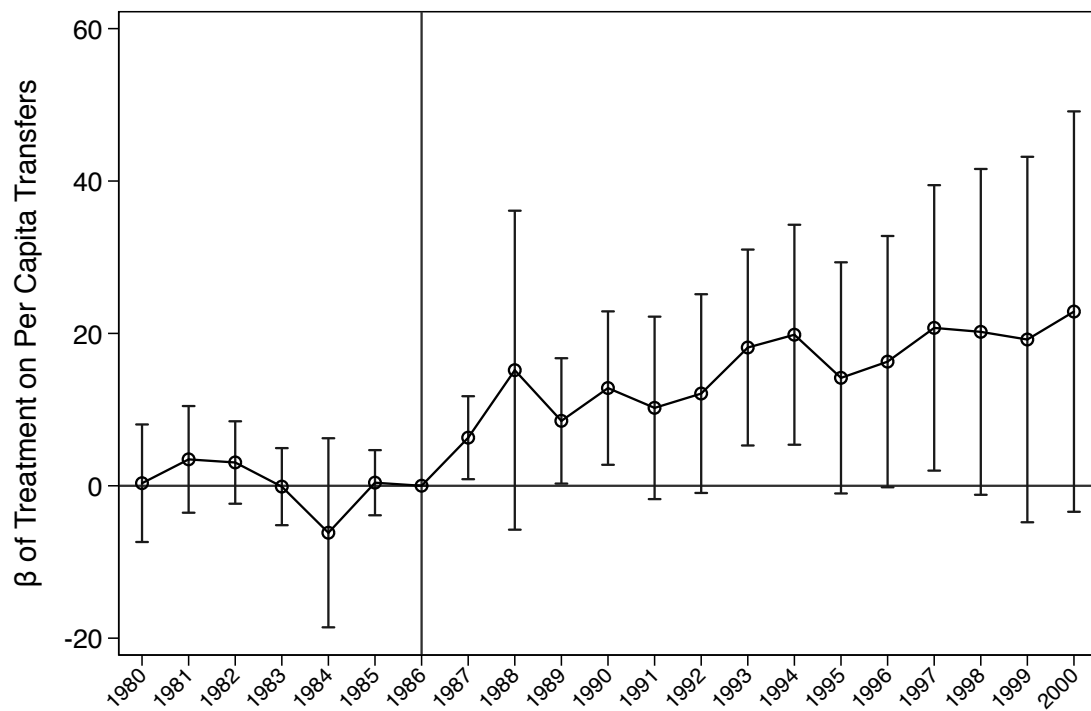
Where  $\ln(y)_{c,t}$  is the natural log of per capita inter-governmental revenue from state to

<sup>14</sup>In Figure A.3 we show the evolution of a number of other county covariates that make clear that the IRCA is not associated with corresponding shocks in such county characteristics as population, income, tax revenue or public school enrolment. This is because undocumented migrants were (and still are) included in population estimates, are eligible (especially their children) for basic public services such as health and education and, to some extent, pay tax as some undocumented migrants obtain illegal social security documents.



**Figure 5:** Evolution of IGR

**Note:** This graph compares per capita inter-governmental revenue received in those counties that never received applications for legal status (control) with those counties that did receive applications for legal status (treated) through the IRCA.



**Figure 6:** Treatment effect interacted with year dummies

**Note:** This graph plots the regression coefficient on a treatment indicator when it is interacted with year dummies as specified in equation 2. The regression only includes county fixed effects. The outcome, like Figure 5, is per capita inter-governmental revenue (in USD1999). Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.  $N = 43,868$ .

local governments (in 1999 USD) in county  $c$  in year  $t$  and  $\delta_c$  is defined as before. The treatment indicator,  $T_c$ , is now interacted with a binary variable  $P_t$ , that is one if  $t \geq 1986$  and zero otherwise. In addition, we include state-by-year fixed effects,  $\zeta_{st}$ , to account for state-specific time-varying shocks that might affect legalisations and transfers, including governor specific characteristics or other state-year-level political or economic shocks.<sup>15</sup> We include a vector of county-level covariates,  $\mathbf{X}_{c,t}$ , that includes poverty and unemployment rates, income and population. As before,  $\epsilon_{c,t}$  is an idiosyncratic disturbance term clustered at the county level.<sup>16</sup>

The trends shown in the raw data are borne out in the regressions. Panel A of Table 2 shows our results across a number of variations of the model shown in 3 and we see precisely estimated coefficients of similar magnitude across a number of specifications. In Panel B, we estimate the same parameters but using a measure of treatment intensity as specified in equation 4. Here,  $\ln(IRCA + 1)_{c,t}$  is the natural log of the cumulative number of IRCA applicants per 1000 county inhabitants (plus one) in county  $c$  in year  $t$ . The parameter of interest,  $\beta_1$ , can be interpreted as the elasticity of state transfers with respect to the cumulative number of per 1000 capita legalised applicants. All other parameters are defined as before.

$$\ln(y)_{c,t} = \delta_c + \zeta_{st} + \beta_1 \cdot \ln(IRCA + 1)_{c,t} + \Theta \cdot \mathbf{X}_{c,t} + \epsilon_{c,t} \quad (4)$$

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<sup>15</sup>In some specifications we drop, or cannot include, state-year-fixed effects and we replace them with year dummies. This is clearly noted in the tables.

<sup>16</sup>Because our unit of observation is the county and our treatment varies at this level, we cluster standard errors at the county. The results, however, are robust to clustering at higher levels, most notably the state. These results are captured in Table A.3 in the Appendix.

**Table 2:** Inter-Governmental Revenue on IRCA Legalisations

	Log of Inter-governmental Revenue (per capita)				
	(1) Baseline	(2) Drop Top 5	(3) Pop < 409,490	(4) Matching	(5) Linear Trends
<i>Panel A. Treatment Indicator</i>					
Treatment × Post	0.0709*** (0.0183)	0.0725*** (0.0199)	0.0493** (0.0193)	0.138*** (0.0314)	0.0556** (0.0241)
<i>Panel B. Treatment Intensity</i>					
Log legalisations	0.0610*** (0.0143)	0.0929*** (0.0217)	0.0448*** (0.0158)	0.0688*** (0.0157)	0.0462*** (0.0173)
Control Variables	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	No	Yes
County-Year Linear Trends	No	No	No	No	Yes
Observations	46,820	43,952	45,132	12,042	46,820
Number of Counties	2,686	2,526	2,612	604	2,686

*Notes:* The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Panel A shows results when using a treatment indicator and Panel B shows results when using a measure of treatment intensity which is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Column 1 is our baseline estimate and suggests that counties affected by the IRCA received, on average, 7 percent more in per capita transfers than those that did not. Given that inter-governmental revenues make up, on average, 30 percent of local revenue, an increase in the order of 7 percent is significant. It corresponds to an increase of one and a half percentage points in the share of revenue received from inter-governmental sources. Using the measure of treatment intensity, the coefficient implies that a 1 percent increase in the number of per capita legalisations in a county is associated with an increase in per capita transfers of about .0610 percent. Within treated counties, moving from the 25<sup>th</sup> to 50<sup>th</sup> percentile of treatment intensity represents a 132 percent increase in per capita legalisations which translates to an increase of about 7 percent in per capita inter-governmental revenue.<sup>17</sup> Because treatment intensity is a much more precise measure of treatment, equation 4 is our preferred specification and henceforth we will use the cumulative number of per 1000 capita IRCA applicants (plus one) as our main explanatory variable.

To ensure that our results are not driven by confounding factors, we undertake a number of sensitivity checks. We begin by re-estimating the parameters of the model in a sample that omits the five most treated states which, in per capita terms, correspond to California, Arizona, Florida, New Jersey and Nevada.<sup>18</sup> As shown in Column 2, the results not only hold but become slightly larger, suggesting that legal status has a greater impact on the distribution of state finances in those states that do not absorb many newly legalised migrants. To alleviate concerns that the results are driven by very populous cities or counties—some of which may serve as so-called ‘sanctuary cities’—we rerun the regression, in Column 3, on a sample restricted only to those counties with populations less than average county population size (i.e. 409,490) and obtain precisely estimated results, albeit of slightly smaller magnitude suggesting that the effect is strongest in more populated counties.

As shown in Figure 3, there are some 330 counties that received applications for legal status and just over 2,000 that did not. One might wonder, therefore, how comparable these two groups of counties are. To address these concerns, we use propensity score matching to generate a more comparable control group. That is, for every county in the sample, we generate, on the basis of its observable characteristics, a propensity score that indicates a given county’s likelihood to be treated. Then, for every treated county, we match the nearest neighbour from the untreated counties to generate a more comparable control group. In Column 4, we rerun the model in this matched sample and obtain results almost identical to those of the baseline.<sup>19</sup>

<sup>17</sup>A very similar increase is associated with moving from the 50<sup>th</sup> to 75<sup>th</sup> percentile of treatment.

<sup>18</sup>Dropping the most treated states in terms of the absolute number of legalisations makes no difference to the results, nor does dropping the four states that border Mexico. These results are not reported.

<sup>19</sup>Changing the number of neighbours up to 5 does not change the result. Figure A.4 in the Appendix shows the trends in inter-governmental revenue in treatment and control counties in the matched sample using nearest neighbour matching. The characteristics on which we generated the propensity score are county income, population, crime, tax revenue, poverty rate and unemployment in 1980. We drop state-year fixed effects to allow for the possibility that the best-matched control county for a given treated county may, in fact, lie in a different state. Matching within a state and leaving state-year fixed effects in the estimation does not change the results.

Finally, in Column 5, we rerun the baseline specification, adding to it county specific linear time trends. The idea here is to capture any differential trends with respect to the outcome variable that might arise over time for each county, trends which might render our identifying assumption implausible. This is the most demanding specification. That the result holds suggests that the relationship between immigrant legalisation and the distribution of state aid is a robust one.

### 5.3 Robustness Checks

In Table 3 we carry out a number of further empirical checks to test the strength of the relationship. In Column 1 we run a first-differences estimation using only two years in the sample: 1982 and 1992. The idea here is to skip intervening years to overcome issues with respect to timing of various sorts: different electoral cycles in different states, different budget response times and different IRCA application processing times. As shown, the legalisation variable maintains its predictive power over per capita inter-governmental revenue.<sup>20</sup> In Column 2, we use a county's 1980 population to carry out all per capita calculations as another way of ensuring population changes are not driving the results. To understand whether the relationship between immigrant legalisation and the distribution of state aid is linear or quadratic, we include a quadratic term of the key explanatory variable in Column 3. As shown, the linear variable retains its precision whereas the quadratic term enters imprecisely. In Column 4 we include quadratic year trends and in Column 5 we include additional county demographic controls, including the share of the population that is over 18 year of age, the share of the population that is Hispanic and the share of county households with children. Column 6 presents results from an instrumental variables estimation which will be explained in more detail in subsection 5.4.

In Table A.4, we replicate the baseline estimates using a *log – linear* specification to demonstrate that the results are insensitive to the logarithmic transformation of the data. We choose a *log – log* specification because (a) the legalisation variable is unevenly distributed and (b) an elasticity is easier for interpretation.

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<sup>20</sup>Figure A.5 plots the coefficients from a number of such regressions, each using a different time period for the difference estimation.



**Table 3: Robustness Checks**

	Log of Inter-governmental Revenue (per capita)					
	(1) $\Delta y_{1992-1982}$	(2) 1980 PC	(3) IRCA <sup>2</sup>	(4) Year <sup>2</sup>	(5) Add. Controls	(6) IV
Log legalisations	0.0777*** (0.0201)	0.0579*** (0.0140)	0.102*** (0.0363)	0.0463*** (0.0179)	0.102*** (0.0177)	0.199*** (0.0635)
Log legalisations <sup>2</sup>			-0.0140 (0.0108)			
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	No	No	No	No	Yes	No
Linear Year Trends	No	No	No	Yes	No	No
Quadratic Year Trends	No	No	No	Yes	No	No
Observations	4,208	41,349	46,820	46,820	6,464	46,810
Number of Counties	2,104	2,211	2,686	2,686	2,407	2,685

*Notes:* The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log legalisations is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). Column 2 carries out the analysis using per capita legalisation and per capita transfers calculated with 1980 county population in the denominator. Additional controls in Column 5 include the share of county population that is over 18, the share of county population that is Hispanic and the share of county households with children, which are only available for 1980, 1990 and 2000. Column 6 uses the share of foreign-born people in a county in 1960 interacted with year dummies as an instrument for log legalisations. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

#### 5.4 Instrumental Variables

As a final empirical test to rule out endogeneity arising out of geographic factors associated with where the undocumented migrants settle, we use the share of a county's foreign-born population in 1960 as an instrument for the number of IRCA applicants post-1986. In doing so, we follow a number of other studies (Hildebrandt et al., 2005; Woodruff and Zenteno, 2007; McKenzie and Rapoport, 2010) that utilise historical rates of migration as an instrument for present levels.

Because the instrument in question is time-invariant we interact the 1960 foreign-born variable with year dummies in order to take advantage of the panel structure of our data. Doing so produces results that are positive and precisely estimated, though an order of magnitude larger than the baseline as shown in Column 6 of Table 3.<sup>21</sup>

#### 5.5 Population Considerations

One may wonder whether our results are simply explained by a mechanical effect of having more people in the population eligible for social programmes. We rule out this possibility for three reasons.

First, while the IRCA legalised approximately 3 million people in some three years, it did not lead to a corresponding increase in the population. This is because estimates of the undocumented population are obtained from a residual of two other population measures: (1) the total foreign-born population (obtained through the Census) and (2) the legally resident population (known by the INS). The undocumented population estimate is the residual when (2) is subtracted from (1); hence population estimates undertaken by the Census Bureau are inclusive of undocumented migrants (Baker and Rytina, 2013). This fact is made evident in Figure A.3 which shows population growth in treated and untreated counties. As illustrated, neither type of county experienced appreciable growth in population in the years before or after the passage of the IRCA. Accordingly, even if funds were transferred by formula on the basis of a county's population, the fact that there is no population growth associated with the IRCA alleviates our concern that mechanical population forces drive our results.

Second, a feature of the IRCA was that it "barred" the newly legalised "from participation in programmes of financial assistance furnished under federal law on the basis of financial need for a period of five years from the effective date of each alien's lawful temporary resident status" (DHHS, December 1991). Moreover, given the demographic characteristics of the newly legalised discussed previously and that the children of undocumented migrants were already

<sup>21</sup>Another option is to run 13 cross-section regressions for each year between 1988 and 2000 where each variable is differenced from its 1982 value, as shown in specification 5, and the differenced variable is then instrumented in the cross-section. The regression coefficients are plotted in Figure A.6 and confirm, both in terms of precision and the timing of the effect, the baseline estimates shown in Figure A.5.

$$\ln(y)_{c,t-1982} = \beta_t \cdot \ln(IRCA + 1)_{c,t-1982} + \Theta_t \cdot X_{c,t-1982} + \epsilon_{c,t} \quad (5)$$

eligible for public services such as schooling pre-IRCA, we find it unlikely that our results are explained by mechanical increases as a result of social assistance eligibility criteria being satisfied at the state level.<sup>22</sup>

Third, the dependent variable used throughout our study is a measure of *per capita* transfers from state to local governments. If the policy was simply associated with a mechanical increase in transfers, we might expect the overall *level* of transfers to increase but there would be no reason, *ex-ante*, to expect any change in the amount of *per capita* transfers. That *per capita* transfers are a function of the number of legalisations in a county seems to suggest that the transfer activity we observe is more than a mechanical increase that might arise out of a transfer formula based on population considerations.

## 5.6 SUTVA

The stable unit treatment value assumption (SUTVA) holds that the potential outcome of a unit of observation is unaffected by the treatment status of other units. In this particular context, therefore, a question arises as to whether counties affected by the IRCA receive their transfers at the expense of those counties not affected by the law or whether these funds come from other sources. To better understand the nature of the treatment effect, and to understand whether SUTVA holds in this particular setting, we undertake two exercises.

First, there are four states in the sample that were unaffected by the IRCA. These are North and South Dakota, Vermont and Wyoming. As a first step, therefore, we run the baseline specification using the treated counties from treated states and only the control counties from these four control states. The idea here is that if the result is reflective of a distributive politics channel where the governor takes from control counties in order to give to treated counties, we should see no effect when we compare treatment and control counties from different states. To compare counties across state borders, we drop state-year fixed effects (and include year fixed effects instead) and generate the results presented in Table 4. They indicate that, by and large, the treatment effect is not coming at the expense of control counties.

To probe this question further, we turn to state revenue data from the Census of Governments. Here we observe a state's revenues from various tax sources as well as from the federal government by way of inter-governmental revenue from the federal government to the state. The coefficient on *per capita* legalisations at the state level shown in Table 5 indicates that revenue from the state increases as a function of IRCA documented migrants in a state.<sup>23</sup>

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<sup>22</sup>Later in the paper, we utilise Census of Government expenditure data to better understand the impact of legal status on various categories of local expenditure and find that the IRCA does not have a significant impact on local welfare expenditure (as shown in Figure 8).

<sup>23</sup>To alleviate concerns that the result is driven by increased inter-governmental revenue from the federal government which simply passing through the state, the Census Bureau explains that "federal aid that is given to the state to then be distributed to local governments is normally considered state aid because states usually have discretion over the distribution."

**Table 4:** The IRCA and SUTVA

	Log of Inter-governmental Revenue (per capita)	
	(1) Full Sample	(2) Control States
Log legalisations	0.0988*** (0.0143)	0.0782*** (0.0177)
Control Variables	Yes	Yes
Year Fixed Effects	Yes	Yes
County Fixed Effects	Yes	Yes
Observations	46,826	10,771
Number of Counties	2,686	749

*Notes:* The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log legalisations is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). In Column 1 we exploit the full sample. In Column 2 we use only treated counties from treated states and the control counties from the four control states in the sample. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table 5:** State Revenues and the IRCA

	Log of State Revenue From...		
	(1) Sales Tax	(2) Income Tax	(3) Federal Gov't
Log Legalisations, State	0.029** (0.013)	0.022* (0.011)	0.013* (0.007)
Control Variables	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Observations	853	853	916
Number of States	41	41	44

*Notes:* The dependent variable is the log of state revenue from various sources. Log legalisations, State is the log of the cumulative number of IRCA applications in a given state in a given year per 1000 state inhabitants (plus one). We control for state unemployment, population and income. Standard errors (shown in parentheses) are clustered at the state level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## 6. Political Economy Mechanisms

The analysis in the preceding section demonstrated that the relationship between immigrant legalisation and the distribution of state finances is a robust one. In this section, we investigate to what extent the relationship is reflective of discretionary, political choices made by state governors and to what extent it is merely reflective of mechanical, bureaucratic forces that oblige the governor to better service the areas where the documented migrants reside. To shed light on these issues, and on the mechanisms that drive the result, we turn our attention to the political constraints on, and the political context of, the state governor. The contention here is simple. If, on the one hand, the increases in per capita transfers associated with the IRCA are the result of mechanical forces, the results ought to be entirely insensitive to political context or constraints. If, on the other hand, the transfers are the result of discretionary choices made by state governors in an effort to bolster political support, then it is not unreasonable to expect state aid to display some sensitivity to political context.

### 6.1 Political Party Heterogeneity

We begin by investigating the sensitivity of our results to the party affiliation of the governor. Column 1 of Table 6 indicates that the per capita transfers a county receives in response to the IRCA policy are positive and significant and that this amount increases by about half when the governor is a Democrat as compared to when he or she is a Republican.<sup>24</sup> In Column 2 we test whether state governors give more to counties that are politically aligned with them, in the sense that a given county's political leaning (measured by its Presidential election results) align with those of the party of the governor.<sup>25</sup> As shown, state aid increases to a county affected by the IRCA regardless of whether the county's political leaning is aligned with that of the governor. Accordingly, these results confirm that the distributional impact of the IRCA is driven more by political factors at the state level.<sup>26</sup>

### 6.2 Term Limits and Election Cycles

Next, because our data includes the names of state governors, we are able to compare state-to-county transfers under a single governor over time as he or she faces different political constraints and election cycles. By way of example, we consider the transfers in just one state,

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<sup>24</sup>Figure A.9 and Table A.6 show, perhaps unsurprisingly, that the Democratic vote share in Presidential elections increases as the share of IRCA migrants in a county increases.

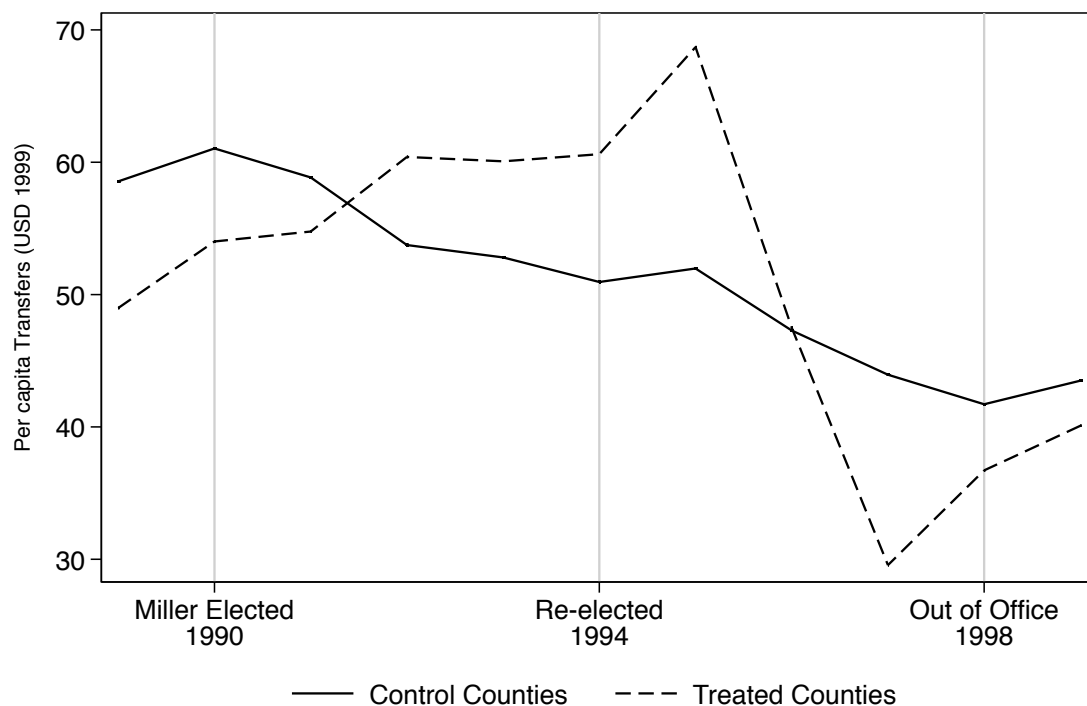
<sup>25</sup>We use Presidential election data as a proxy for Gubernatorial electoral returns because the Gubernatorial election data is available only as of 1990, after the variation in legalisations has ended. A county's Presidential election outcomes do follow its Gubernatorial outcomes quite well as shown in Figure A.8.

<sup>26</sup>In Table A.7 we explore whether the partisan composition of Congress has any explanatory power over the results. To this purpose, we interact the legalisation variable with indicators for whether a given state's Members of Congress or Senators were majority Democrats or not. As shown, the party affiliation of a states federal representatives has no explanatory power on the overall manner in which the state budget is distributed in response to IRCA.

**Table 6:** Legalisation and Political Heterogeneity

	Log of Inter-governmental Revenue (per capita)			
	(1) Party	(2) Aligned	(3) Incentive	(4) Election Cycle
Log legalisations	0.0516*** (0.0160)	0.0544*** (0.0143)	0.0420*** (0.0143)	0.0527*** (0.0157)
D-Governor × Log legalisations	0.0234* (0.0121)			
Aligned		-0.000185 (0.00754)		
Aligned × Log legalisations		0.0139 (0.00936)		
Log legalisations × Incentive			0.0246** (0.0107)	
Log legalisations × Election Year				0.0188*** (0.00706)
County Controls	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	46,393	46,820	41,807	30,694
Number of Counties	2,686	2,686	2,677	2,381

*Notes:* The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. *Log legalisations* is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). *D-Governor* is an indicator that is 1 if the party of the governor is Democratic and 0 if Republican. *Aligned* is an indicator that is 1 if the county's election results in the most recent Presidential election (Democrat or Republican) are aligned with the party of the Governor and 0 if not. *Incentive* is an indicator that is 1 if a governor is not a lame duck and 0 otherwise. *Election Year* is an indicator according to whether a governor is in an election year or not. The baseline effects of *D-Governor*, *Incentive* and *Election Year* are captured by state-year fixed effects and are thus unable to be estimated. The outcome variable in Column 4 is lagged by one year. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$



**Figure 7:** Transfers in Zell Miller's Georgia

**Note:** This graph plots inter-governmental transfers in 1999 USD in Georgia during Governor Zell Miller's time in office. Control counties are those that never received applications for legal status through the IRCA whereas treated counties did receive applications for legal status through the IRCA. Georgia comprises 157 counties of which we have data on 137: eight affected by the IRCA and 129 were not.

Georgia, over the political career of one of its governors, Zell Miller (D), from 14 January 1991 to 11 January 1999. Governor Miller served two terms in office: from 1990 to 1994 and from 1994 to 1998. Georgia has a two-term limit constraint on the Governor.<sup>27</sup> Therefore Zell Miller was eligible for re-election in his first term but he was a lame duck in his second. Georgia comprises 159 counties of which we have data for 137. From among the counties for which we have data, eight received legalised migrants as a result of the IRCA and 129 did not. Figure 7 shows the trends in transfers during Zell Miller's tenure as Governor. As shown, the counties that received no legalisations experienced a steady decline in the amount of per capita transfers received. The eight counties that received legalisations, by contrast, exhibit a great deal of variation. In Governor Miller's first term, transfers to these counties increase only to drop off drastically in his second term when he is no longer eligible for re-election. The question that arises, therefore, is to what extent inter-governmental revenue differs as governors face term limits and how much of this difference is driven by the IRCA policy. Similarly, one wonders to what degree state aid fluctuates in the face of gubernatorial election cycles.

<sup>27</sup>In the United States 26 states had term limits from 1980 to 1986, the majority of which were limited to 2 terms. Thereafter, the number of states with term limits increased to over 30, again the vast majority with a 2 term limit.

The regressions in Table 6 investigate these questions. In Column 3, we interact the legalisation variable with a binary variable that is one when the governor is eligible for re-election and zero when (s)he is a lame duck owing to a term limit and the result indicates that the difference in transfers between lame duck and non-lame-duck governors is indeed positive and significant.<sup>28</sup> We carry out a similar analysis in Column 4, this time analysing sensitivity to the gubernatorial election cycle. Here, we lag the outcome variable by one year to better understand the dynamics of inter-governmental revenue in the year prior to an election. The result suggests that counties affected by the IRCA receive about 35 percent more in inter-governmental revenue in the year prior to a gubernatorial election.

### 6.3 Electoral Competition

To shed further light on mechanisms, we examine the sensitivity of transfers to electoral competition. The logic is similar to those of term limits. If the transfers we observe are discretionary, we would expect more resources to flow into those counties whose previous electoral races have been more competitive. To test this hypothesis, we generate the absolute value of the winning margin between Democrats and Republicans in the 1984 and 1988 Presidential election and identify those counties whose win margins are tighter than the tightest 25, 10 and 5 percent of the distribution in both elections. We then interact the legalisation variable with an indicator for whether a given county is competitive and compare this interaction across two time periods: 1984 to 1988 when the IRCA migrants were ineligible to vote and 1988 to 1992 when the IRCA migrants were eligible to vote. Results are shown in Table 7 and indicate that the impact of legalisation on state-to-county transfers is amplified in the post-1988 period when a given county is more politically contested.

### 6.4 Veto Power and State Legislatures

As mentioned earlier in the paper, governors exercise strong influence over the budget-making process in a given state. In this subsection, we focus on arguably the most influential of these powers: the line-item veto.<sup>29</sup> This accords with a range of theoretical and empirical literature that documents the growing importance of the state executive branch relative to the legislative branch in setting state priorities in general (Clych and Lauth, 1991) and in shaping the state budget in particular (Kousser and Phillips, 2012; Barrilleaux and Berkman, 2003).

In Columns 1 and 2 of Table 8, we split the sample according to whether or not the state

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<sup>28</sup>The advantage of this approach is that it allows us to trace the evolution of transfers over the course of a single governor's term. However, one might be concerned that this specification does not allow us to estimate a governor's electoral incentive arising from the IRCA since the IRCA ceases to produce meaningful variation in the number of documented migrants after 1992. To address this, we re-estimate the parameter of interest, limiting the sample to the period only between 1989 and 1994 and compare governors who are lame ducks in this period with those who are not. Results are shown in Table A.8 and indicate that governors with an electoral incentive allocate significantly more resources than their lame duck counterparts as the share of documented migrants in a county increase.

<sup>29</sup>Figure A.10 illustrates how this power has grown stronger over time.



**Table 7:** Legalisation and Tightness-of-the-Race

	Log of Inter-governmental Revenue (per capita)		
	(1) Tightest 25%	(2) Tightest 10%	(3) Tightest 5%
Log legalisations	0.103*** (0.0353)	0.0954*** (0.0324)	0.0929*** (0.0322)
Log legalisations × Tight25 × Post-1988	0.0698* (0.0364)		
Log legalisations × Tight10 × Post-1988		0.0736* (0.0377)	
Log legalisations × Tight5 × Post-1988			0.0688** (0.0350)
Control Variables	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
Observations	18,585	18,585	18,585
Number of Counties	2,561	2,561	2,561

*Notes:* The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log legalisations is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). Tight25, Tight10 and Tight5 indicate, respectively, whether the outcome of the 1984 and 1988 Presidential election in a given county was more competitive (defined as the absolute difference between votes for the Republican and Democratic candidate) than those in the 25<sup>th</sup>, 10<sup>th</sup> and 5<sup>th</sup> percentile of the competitiveness distribution. Post-1988 is 1 for the period between 1988 and 1992 and 0 for the period from 1984 to 1988. The baseline effects of Tight25, Tight10, Tight5 are captured by county fixed effects. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

governor enjoys line-item veto power and estimate the parameters of our baseline specification.<sup>30</sup> As shown, the legalisation variable has strong predictive power over inter-governmental revenue in those states where the governor has line item veto power.

In Column 3 of Table 8, we test the sensitivity of transfers to the relationship between the state governor and the state legislature. Although governors do enjoy increasing power over the state budget, legislatures still play a role. And if the result is discretionary, as opposed to mechanical, we might expect it to display important heterogeneities depending on the relationship between the executive and legislative branches of state government. Accordingly, we generate an indicator that is one when the party of the governor is aligned with the partisan majority of the state legislature and zero otherwise.<sup>31</sup> The result indicates that, although transfers increase as the share of IRCA applicants in a county increases when there is no political alignment between the governor and the legislature, the result increases by about 50 percent when there is political alignment, further underscoring the politically discretionary nature of these transfers.

## 6.5 Re-election Considerations

How might these political economy results impact a governors re-election chances? Our data includes an indicator for whether a particular governor was re-elected and we exploit this variable to understand whether the share of documented migrants in a state affects re-election chances in any way. Because this outcome varies at the state level over time, we can only include state and year fixed effects separately, denoted by  $\gamma_s$  and  $\alpha_t$  respectively, but not jointly. Moreover, we allow for a five year lag on the key independent variable to take into account the time it takes for the documented migrants to earn the right to vote. Our specification is thus expressed in equation 6, where  $R_{s,t}$  is an indicator for whether the governor in state  $s$  has been re-elected in year  $t$ .

$$R_{s,t} = \gamma_s + \alpha_t + \beta_1 \cdot \ln(IRCA + 1)_{s,t-5} + \Theta \cdot \mathbf{X}_{s,t} + \epsilon_{s,t} \quad (6)$$

The result is presented in Column 4 of Table 8. It suggests that as the share of documented migrants in a state increases, so too does the governors chances for re-election, lending further credence to the political economy nature of our baseline results.<sup>32</sup>

<sup>30</sup>Specifically, we compare states where the governor has line-item veto power to states where the governor has a simple veto, but not line-item veto.

<sup>31</sup>This includes when the state legislature is split or has a majority of the opposite party to the governor.

<sup>32</sup>One question that might arise is how visible these transfers are in the sense of their ability to influence public opinion about the incumbent. In this respect, an assumption in our conceptual framework is that voting decisions in a county are based on local economic conditions and that voters base their decision not just on local conditions immediately prior to the election but rather throughout the term of the governor. In the terminology of Elinder, Jordahl and Poutvaara (2015), voters in our framework base their decisions *retrospectively* (i.e. based on the implemented policies of the incumbent) rather than *prospectively* (i.e. based on the promises candidates make). In Table A.9 we investigate how the allocation on state aid affects local spending and the coefficient suggests that across all categories of local spending, the elasticity of such spending with respect to state aid is positive and

**Table 8:** Veto, State Legislatures and Re-election

	(1) Veto	(2) No Veto	(3) Alignment	(4) Re-elected
Log legalisations	0.0640*** (0.0131)	0.00709 (0.0301)	0.0535*** (0.0139)	
Log legalisations × Alignment			0.0245** (0.0109)	
Log legalised <sub>t-5</sub> , State				0.247*** (0.0545)
County controls	Yes	Yes	Yes	No
State controls	No	No	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	No
State-Year Fixed Effects	Yes	Yes	Yes	No
State Fixed Effects	No	No	No	Yes
Observations	41,449	5,356	46,820	6,091
Number of Counties	2,555	670	2,686	2,239

*Notes:* In Columns 1 to 3, the dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. In Column 4, the outcome is a binary variable that is 1 if the governor is re-elected and 0 otherwise. Log legalisations is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one) and Log legalised<sub>t-5</sub>, State is similarly defined, but aggregated at the state level and with a five year lag. Column 1 restricts the sample to those Governors who enjoy line-item veto power and Column 2 restricts the sample to those Governors who enjoy no such power. Alignment is an indicator that is one when the party of the Governor is aligned with the partisan majority of the state legislature and 0 when it is not. The baseline effects of Aligned is captured by state-year fixed effects and thus cannot be estimated. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. State controls are the same, but aggregated to the state level for the specification in Column 4 and also include the party of the governor. Standard errors (shown in parentheses) are clustered at the county level in Columns 1 to 3 and at the state level in Column 4. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## 7. Capturing the Vote of the Newly Legalised

Thus far, we have demonstrated a robust relationship between immigrant legalisation and the distribution of public resources. Governors allocate more resources to counties as the share of newly documented migrants in those counties increase, an allocation which is particularly sensitive to the political context of the incumbent. Until now, however, we have implicitly assumed that the inter-governmental revenue is intended to capture the vote of the migrants who obtained legal status through the IRCA. Of course, because the inter-governmental revenue we observe is aggregated at the county level, we are unable to verify with certainty which constituents these monies are actually intended to reach.<sup>33</sup> In this section, therefore, we present a number of pieces of evidence that demonstrate that it is indeed the IRCA migrants who motivate the state governor.

### 7.1 Attitudes Towards Migrants

Like today, undocumented migration was a politically charged issue at the time of the IRCA. A notable opponent of the IRCA, and of undocumented migration more generally, was Governor Pete Wilson, Republican governor of California, who ran a campaign of fear and anti-migrant propaganda.<sup>34</sup> In his 1994 re-election campaign, Governor Wilson pinned his hopes onto Proposition 187, the “Save Our State” ballot initiative, and the Republican Party offered ideological and financial backing to see the proposition go through. Proposition 187 prohibited undocumented migrants from using non-emergency public services and required the providers of such services to immediately report undocumented migrants for deportation. It was passed by California’s voters only to be struck down by a federal court. The proposition, and Wilson’s campaign to support it, was highly controversial and left somewhat of an enduring legacy. Bowler, Nicholson and Segura (2006), for example, find that racially charged ballot initiatives in California—and specifically Proposition 187—are significantly associated with a shift in political support away from the Republican party and towards the Democratic party on behalf of non-Hispanic white voters as well as Latino voters.

In light of this political context, it seems reasonable to ask to what degree our results are actually driven by governors catering to anti-migrant sentiment arising out of the IRCA rather than to the needs of the documented migrants themselves. We examine this question first by

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precisely estimated, indicating that these transfers are visibly felt at the local level.

<sup>33</sup>Moreover, because this revenue is dedicated to such purposes as health and road improvements, the governor can use it to win over several constituents and not just a single one. Indeed, a key difference between the governor and his or her legislative counterparts is that the governor can shape a states fiscal priorities so as to build winning coalitions from among otherwise competing constituents; legislators on the other hand often vote over single issues, increasing the likelihood of generating ‘winners’ and ‘losers’ from any given vote (Cascio and Washington, 2014).

<sup>34</sup>In a dramatic re-election advertisement, Governor Wilson states “I’m suing to force the Federal Government to control the border and I’m working to deny state services to illegal immigrants. Enough is enough.” (Transcribed from the Television Ad which can be found at: <https://www.youtube.com/watch?v=1LIzZs2HHgY>. Accessed 8 March 2018.

**Table 9: IRCA and Anti-Migrant Sentiment**

	Log of Inter-governmental Revenue (per capita)		
	(1) Wilson	(2) No Cali	(3) No Anti-Migrant States
Log legalisations	-1.844 (1.601)	0.0834*** (0.0182)	0.0579** (0.0238)
Log legalisations × Prop 187 VS	0.0372 (0.0241)		
Control Variables	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
Observations	428	45,690	32,232
Number of Counties	54	2,632	1,864

*Notes:* The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log legalisations is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). Prop187 VS is the county vote share for Proposition 187. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Column 3 excludes the 10 states (plus California) that passed ballot initiatives or laws similar in spirit to those of Proposition 187. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

quantifying the impact of Governor Wilson’s term in office and of Proposition 187 on state aid. In Column 1, therefore, we restrict the sample to consider only California during the eight years for which Governor Wilson was in power (1991 to 1998) and exploit variation in county-level voting outcomes for Proposition 187. Forty-seven of California’s 55 counties voted for the Proposition and eight rejected it and the results varied from as little as 29 percent in favour to as much as 77 percent. Perhaps unsurprisingly, counties more affected by the IRCA received less inter-governmental revenue during Governor Wilson’s tenure. However, this result wiped away and made positive for counties with a vote share of 49.5 percent or higher. However, neither of the coefficients are precisely estimated, which suggests that the impact of immigrant legalisation on state aid is not, in California at least, confounded by anti-migrant sentiment. In Column 2 we estimate the parameters of the baseline specification excluding California, the state with the strongest expression of anti-migrant sentiment at the time and the results hold. In the years following proposition 187, ten other states passed ballot initiatives or laws similar to that of Proposition 187.<sup>35</sup> Dropping these states from the analysis—presumably the states where governors had the strongest incentives to cater to anti-migrant sentiment—does not alter the results in any meaningful way.

<sup>35</sup>These are Arizona, Colorado, Florida, Georgia, Illinois, Nevada, New Mexico, New York, Oklahoma and Texas as reported by Richard Lacayo (December 19, 2004) in the following report: <https://ti.me/2PbD7YE>.

**Table 10:** The IRCA and Attitudes towards Migration (GSS Survey)

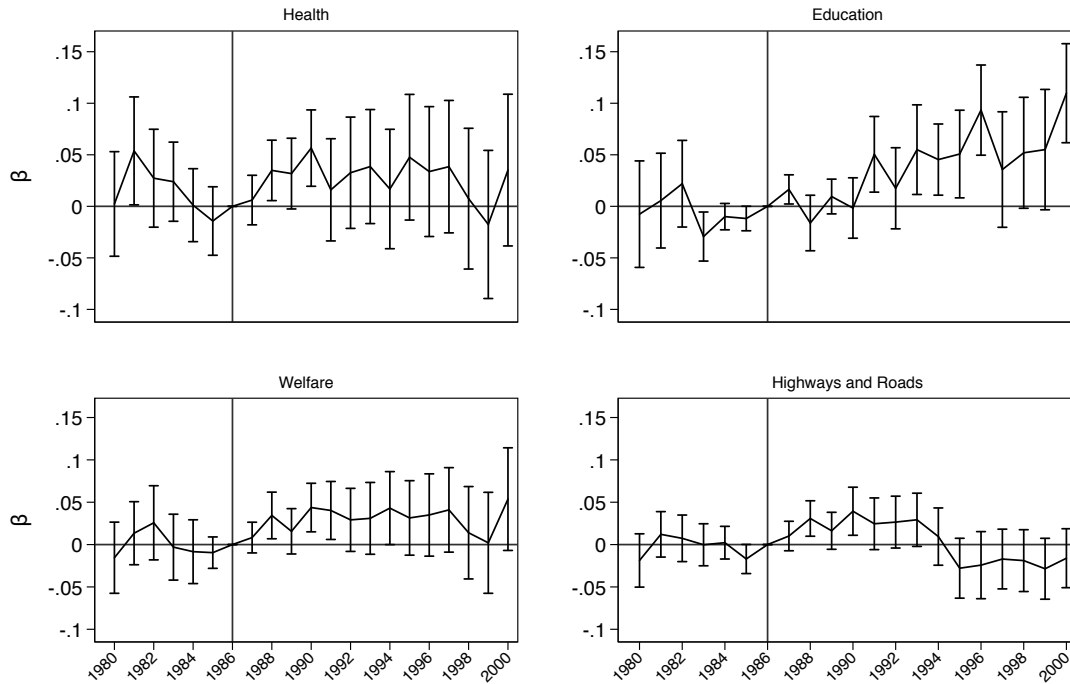
	Attitudes on Undocumented Migrants			Attitudes on Documented Migrants	
	(1) Given Work Permits	(2) Work Hard	(3) Deported	(4) Increase Crime	(5) Take Jobs Away
Log legalisations	1.669** (0.718)	0.540*** (0.130)	-1.818* (0.815)	-3.673* (1.698)	-3.760* (1.858)
Control Variables	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	No	No	No
Region Fixed Effects	No	Yes	No	No	No
Observations	873	16,179	705	581	639
Number of Regions	9	9	9	9	9
Years in Sample	1994	1980 — 1998	1996	1996	1996

*Notes:* This table uses General Social Survey (GSS) data merged with the legalisation data. Log legalisations is the log of the cumulative number of IRCA applications in a given region in a given year per 1000 region inhabitants (plus one). The outcome variables are all binary indicators on various attitudes towards documented and undocumented migrants. Control variables include individual income, employment status, marital status, age, educational attainment and race. Standard errors (shown in parentheses) are clustered at the region level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

As a more general check, we merge the legalisation variable with data from the General Social Survey (GSS), which includes a range of questions on attitudes towards migration. As indicated in the various columns of Table 10, a higher share of legalised migrants in a region tends to improve, not worsen, attitudes towards documented and undocumented migrants. Individuals residing in such regions tend to think undocumented migrants work hard, deserve work permits and ought to be protected against deportation. Such individuals are also of the opinion that documented migrants neither increase crime nor take jobs away from native citizens. Given these attitudes, and given the fact that the IRCA was not associated with an influx of migration but rather a change in the legal status of already resident migrants, we find it unlikely that state aid in IRCA-affected counties is intended to satisfy nativist sentiment or general opposition to the amnesty.

## 7.2 The IRCA, Local Expenditure and Hispanic Outcomes

Finally, we turn our attention from county revenue to county expenditure in an effort to better understand in which areas and, potentially, on which constituents county revenue is spent. Figure 8 plots event study estimates when the legalisation intensity in 1992 is interacted with year dummies in regressions with various categories of local expenditure as the outcome. These figures suggest that the IRCA led to increases in local expenditure in the areas of health, education and welfare but that these increases are estimated with precision for education expenditure beginning in 1991.



**Figure 8:** Event study estimates of local expenditure on legalisation

**Note:** This graph plots the regression coefficient on the log of the cumulative number of IRCA applications in a given county per 1000 county inhabitants (plus one) in 1992 when it is interacted with year dummies. The outcome variables are the log of per capita county expenditure in health, education, welfare and highways and roads. The regressions control for poverty and unemployment rates, log of population and log of income, all aggregated to the county level as well as county and state-by-year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.  $N = 34,840$  for all regressions.

To understand whether these educational expenditures were intended to benefit the newly documented migrants and/or their families, we calculate race-specific high school completion rates to test whether the counties that were affected by the IRCA also experienced improvements in Hispanic high school completion. To carry out this exercise, we obtain data from the 2010 decennial census in order to estimate the impact of the IRCA on an individual's educational outcomes. Rather than compare individuals in treated and non-treated counties before and after the passage of the IRCA, we now compare individuals in treated and non-treated counties in cohorts that entered middle school before the passage of the IRCA (and hence were less likely to benefit from additional educational expenditure) with those cohorts that entered middle school after the IRCA passed (and hence were more likely to benefit from additional funds). Accordingly, we construct 13 middle school entry cohorts from 1980 to 1992. An individual in the 2010 census is placed in a middle school entry cohort depending on which year he or she was 12 years of age. The specification is detailed in equation 7, where  $H_{i,c,mse}$  is an indicator if individual  $i$  in county  $c$  and in middle school entry cohort  $mse$  has 12 years of education or more. County and middle school entry cohort fixed effects are captured by  $\delta_c$  and  $\psi_{mse}$ , respectively and  $D_{mse}^j$  is an indicator that is one when  $j = mse$  and zero otherwise  $\forall j \neq 7$ . All



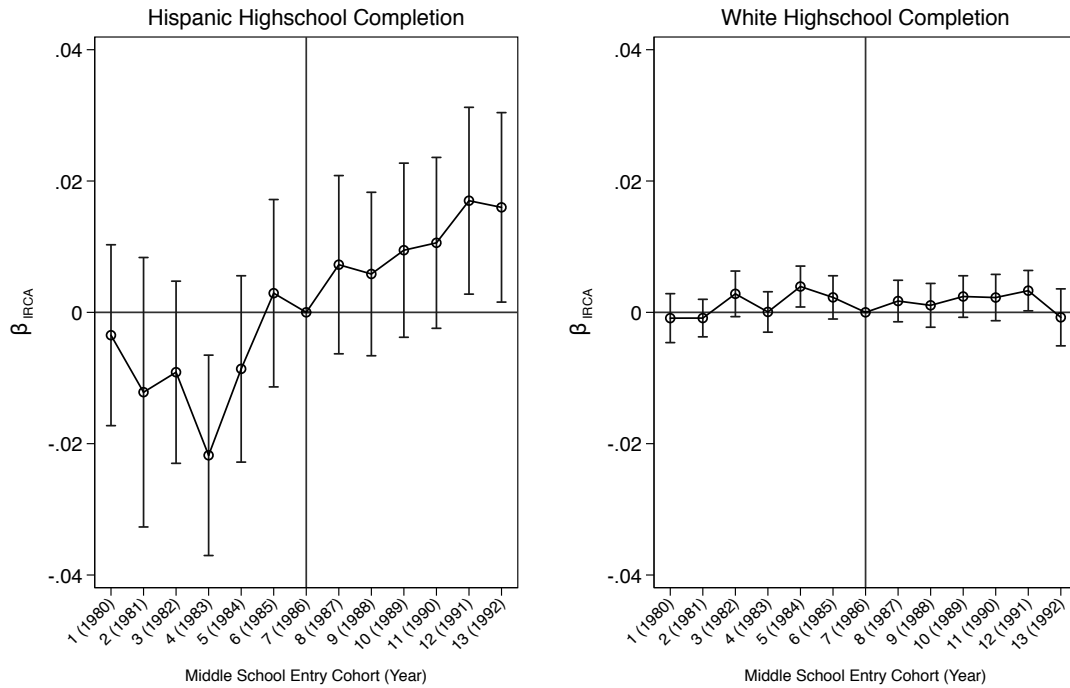
other terms are defined as before.

$$H_{i,c,mse} = \delta_c + \psi_{mse} + \sum_{j=1}^{13} \beta_j [\ln(IRCA + 1)_{c,1992} \times D_{mse}^j] + \epsilon_{c,mse} \quad (7)$$

We first run the specification on a sample of only Hispanic individuals and then on a sample of only Caucasian persons and plot the corresponding coefficient,  $\beta_j$ , as shown in Figure 9. This coefficient estimates the change in the slope of high school completion between individuals in high and low treatment intensity counties across various middle school entry cohorts. The event study estimates indicate that for Hispanic persons, residing in a county affected by the IRCA led to a positive and significant impact on that person's likelihood of completing high school, provided they entered middle school in 1991 or 1992. Indeed, there is no distinguishable difference in the likelihood of completing high school between individuals in high-treated and low-treated counties if they began middle school prior to this time. The timing of this effect suggests that the increased high school completion rates arise not just from legal status but from additional resources that these counties receive for education. For Caucasians individuals, by contrast, residing in an IRCA-affected county has no distinguishable impact on high school completion probability, regardless of when they began middle school. These results lend further credence to our hypothesis that state politicians targeted newly documented migrants.<sup>3637</sup>

<sup>36</sup>The results are even stronger when comparing individuals across different elementary school entry cohorts (i.e. the year in which an individual was six years of age) and are shown in Figure A.13.

<sup>37</sup>In the Appendix, we also try to understand the electoral relevance of the IRCA. To address this, we obtain individual-level data from the November voter supplement of the CPS which includes indicators for whether a person voted in the November election. Our aim is to understand how individual voting is affected by both the IRCA and inter-governmental revenue. We thus plot the marginal effect of the IRCA on the propensity of an individual to vote along the distribution of inter-governmental revenue as shown in Figure A.12. Importantly, though, we have data for only three periods: 1996, 1998 and 2000 because the CPS does not include county identifiers for earlier time periods. As such, these results include no pre-treatment observations and ought to be taken as suggestive. Table A.10 presents the results of the margins plot in table form along with other results from the CPS.



**Figure 9:** Event study estimates of high school completion on legalisation

**Note:** This graph plots the regression coefficient on legalisation intensity in 1992 when it is interacted with middle school entry cohort dummies as shown in equation 7. A person is placed in a middle school entry cohort depending on the year in which they were 12 years of age. The outcome variable, taken from the 2010 decennial census, is an indicator that is one if an individual in a given county and middle school entry cohort completed high school or more and zero otherwise. The regressions include county and cohort fixed effects. The panel on the left plots coefficients when the sample is restricted only to Hispanic individuals whereas the figure on the right estimates the coefficients on a sample of only Caucasian individuals. Standard errors are clustered at the county level and confidence intervals are drawn at 90 percent. For the Hispanic sample,  $N = 52,222$  whereas for the Caucasian sample  $N = 133,907$ .

## 8. Conclusion

Undocumented migration in the United States has become a deeply polarised issue. In this paper, we set out to investigate the distributional consequences of giving undocumented migrants legal status through a nation-wide amnesty programme. Our contention has been that state governors allocate more resources to those counties where the newly documented migrants reside in an effort to win over their future political support. We substantiated this hypothesis in parts.

First, we found that documenting migrants does indeed have a significant distributional component. Across a number of specifications, our results consistently demonstrate that as the share of documented migrants in a county increases, so too does the amount of per capita aid received by that county from its respective state government.

Second, in trying to understand why legal status affects the distribution of public finances, we uncovered political economy forces at work. We presented evidence that the allocation of state aid that arises out of the IRCA varies significantly according to the political context

in which an incumbent governor finds him or herself. Governors transfer more resources to IRCA-affected counties when the governor is eligible for re-election, when the county is more politically contested, when the governor enjoys line-item veto power over the state budget and when the legislative and executive branches of state government are politically aligned. These results are especially noteworthy as it suggests that the relationship between legal status and the distribution of public resources is one of discretionary political choice rather than one of economic necessity or mechanical welfare increases.

In the final part of our analysis, we addressed the question of whether—and to what extent—state governors actually targeted resources to capture the political support of the newly documented migrants rather than that of other, perhaps competing, voting groups. In this respect, we exploited data from a key anti-migrant ballot measure as well as from survey data on attitudes and found little evidence of anti-migrant sentiment confounding our results. Lastly, we found that county expenditure in education increases significantly in IRCA-affected counties and that, consequently, Hispanic individuals, as compared to Caucasian ones, residing in those same counties experience significant improvements in educational outcomes, further suggesting that the resource allocation arising out of the IRCA is intended to service the needs, and win the political support, of the newly documented migrants.

On the whole, then, our findings point to a significant political economy dimension to immigrant legalisation. Offering legal status not only leads to various social and economic improvements at the local level but also provides politicians with strong electoral incentives to see that it does so.

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## Appendix A

### A.1 Analysis of the Model

Our analysis begins by taking first order conditions of equation 1 with respect to  $g$ :

$$\left. \frac{\partial \Pi}{\partial g} \right|_{g=g^*} = (1 - \alpha)U'_C(g^*) + \alpha U'_L(g^*) + E \cdot \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial \phi}{\partial g^*} - C'(g^*) \stackrel{!}{=} 0 \quad (8)$$

To understand how  $g^*$  responds to a sudden shock in legal status,  $\alpha$ , we maximize 8 and this is implicitly given by the following:

$$\frac{\partial g^*}{\partial \alpha} = - \frac{U'_L(g^*) - U'_C(g^*) + E \cdot \frac{\partial \frac{\partial \Omega}{\partial \phi} \frac{\partial \phi}{\partial g}}{\partial \alpha}}{\left. \frac{\partial^2 \Pi}{\partial g^2} \right|_{g=g^*}} \quad (9)$$

Because  $\frac{\partial^2 \Pi}{\partial g^2} < 0$  the sign in front of equation 9 becomes positive. Moreover, we have assumed that  $U'_L(g) > U'_C(g) \forall g$ ; accordingly, the first term in the numerator,  $U'_L(g^*) - U'_C(g^*) > 0$ . The overall sign of equation 9 thus hinges on the sign of the second term in the numerator which can be expressed as follows:

$$\frac{\partial \frac{\partial \Omega}{\partial \phi} \frac{\partial \phi}{\partial g}}{\partial \alpha} = \frac{\partial \frac{\partial \Omega}{\partial \phi}}{\partial \alpha} \cdot \frac{\partial \phi}{\partial g} + \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial \frac{\partial \phi}{\partial g}}{\partial \alpha} \quad (10)$$

Rewriting  $\frac{\partial \frac{\partial \Omega}{\partial \phi}}{\partial \alpha} = \frac{\partial \frac{\partial \Omega}{\partial \phi}}{\partial \alpha} \cdot \frac{\partial \phi}{\partial \phi} = \frac{\partial^2 \Omega}{\partial \phi^2} \cdot \frac{\partial \phi}{\partial \alpha}$ , and  $\frac{\partial \frac{\partial \phi}{\partial g}}{\partial \alpha} = \frac{\partial \frac{\partial \phi}{\partial g}}{\partial \alpha} \cdot \frac{\partial g}{\partial g} = \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha}$  we can substitute these back into equation 10 to obtain:

$$\begin{aligned} &= \frac{\partial^2 \Omega}{\partial \phi^2} \cdot \frac{\partial \phi}{\partial \alpha} \cdot \frac{\partial \phi}{\partial g} + \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha} \\ &= \frac{\partial^2 \Omega}{\partial \phi^2} \cdot \frac{\partial \phi}{\partial \alpha} \cdot \frac{\partial \phi}{\partial g} \cdot \frac{\partial g}{\partial g} + \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha} \\ &= \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha} \cdot \left( \frac{\partial^2 \Omega}{\partial \phi^2} + \frac{\partial \Omega}{\partial \phi} \right) \end{aligned}$$

Under the assumption that  $\frac{\partial \Omega}{\partial \phi} > \left| \frac{\partial^2 \Omega}{\partial \phi^2} \right|$ , the overall sign of equation 10 is thus positive. This in turn allows us to state that  $\frac{\partial g^*}{\partial \alpha} > 0$ .<sup>38</sup>

<sup>38</sup>Although we have used a general functional form for  $\Omega$ , for illustrative purposes, we set  $\Omega(\phi) = \frac{1}{1+e^{-\phi}}$  and plot the various derivatives of  $\Omega(\phi)$ , shown in Figure A.1, to provide some intuition behind this assumption.

**Prediction 1:** The optimal allocation of state aid increases in the share of newly documented migrants in a county.

Given that the second term in the numerator in equation 9 is positive, we can state that

$$\left. \frac{\partial g^*}{\partial \alpha} \right|_{E=1} > \left. \frac{\partial g^*}{\partial \alpha} \right|_{E=0}.$$

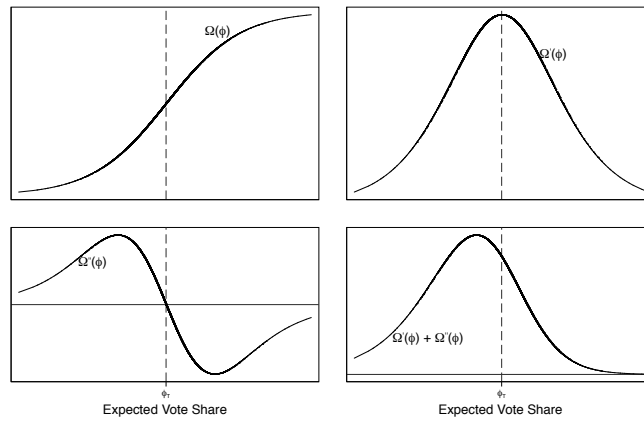
**Prediction 2:** The optimal allocation of state aid is larger when  $P$  is eligible for re-election and less when (s)he is a lame duck.

Finally, the functional form of  $\Omega(\phi)$  leads us to a final testable prediction. Because  $\phi = \phi_T$  represents an inflection point (where  $\phi_T$  represents the winning threshold of an election), it follows that  $\frac{\partial \Omega}{\partial \phi}$  is maximized as  $\phi \rightarrow \phi_T$ .

**Prediction 3:** The optimal allocation of state aid becomes larger the closer the  $P$ 's expected vote share is to the winning threshold.

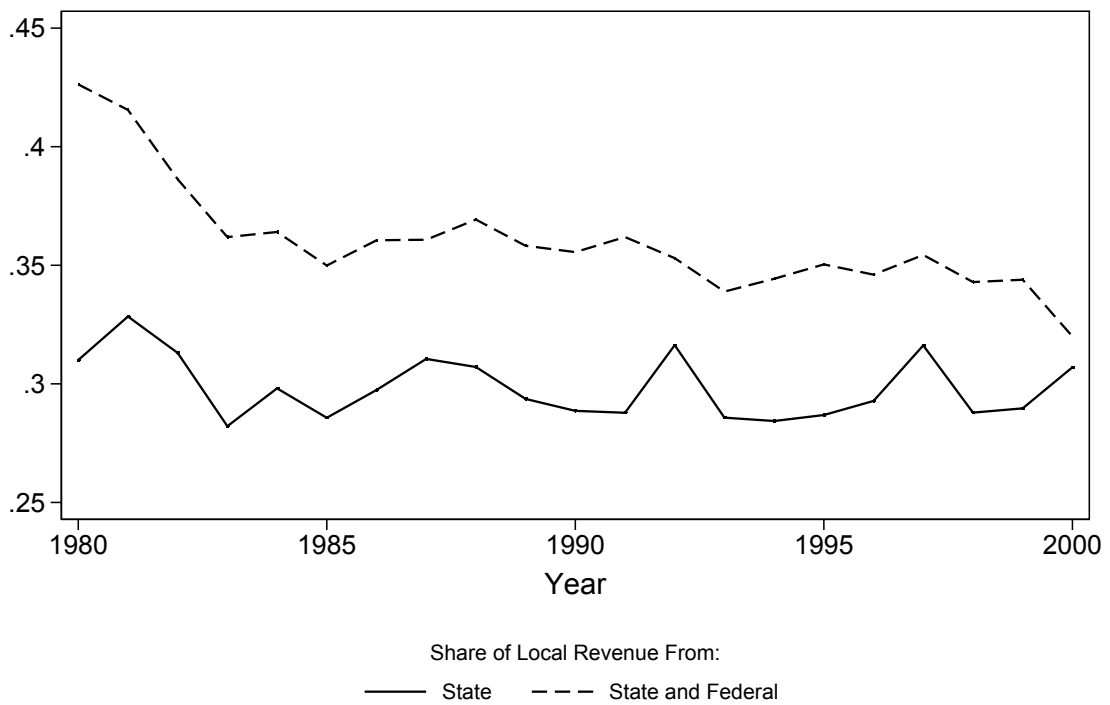


B.2 Additional Figures



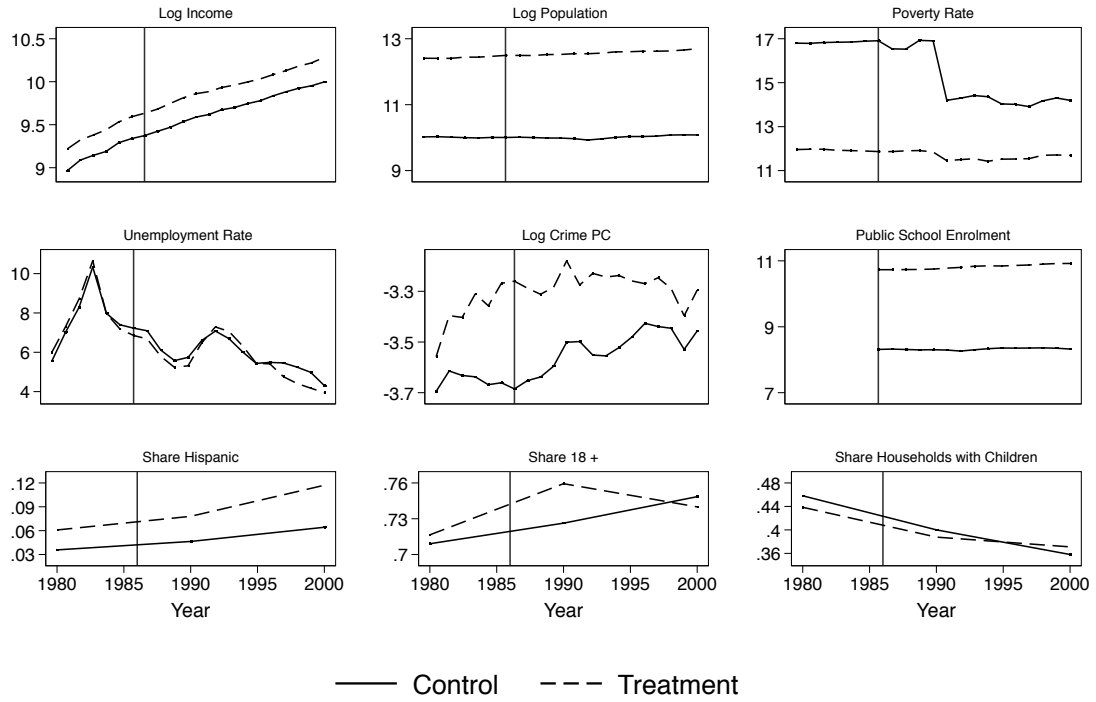
**Figure A.1:** Functional form of  $\Omega(\phi)$ , its first and second derivative and their sum

**Note:** This graph plots, clockwise from top-left:  $\Omega(\phi)$ ;  $\Omega'(\phi)$ ;  $\Omega''(\phi)$ ; and  $\Omega'(\phi) + \Omega''(\phi)$



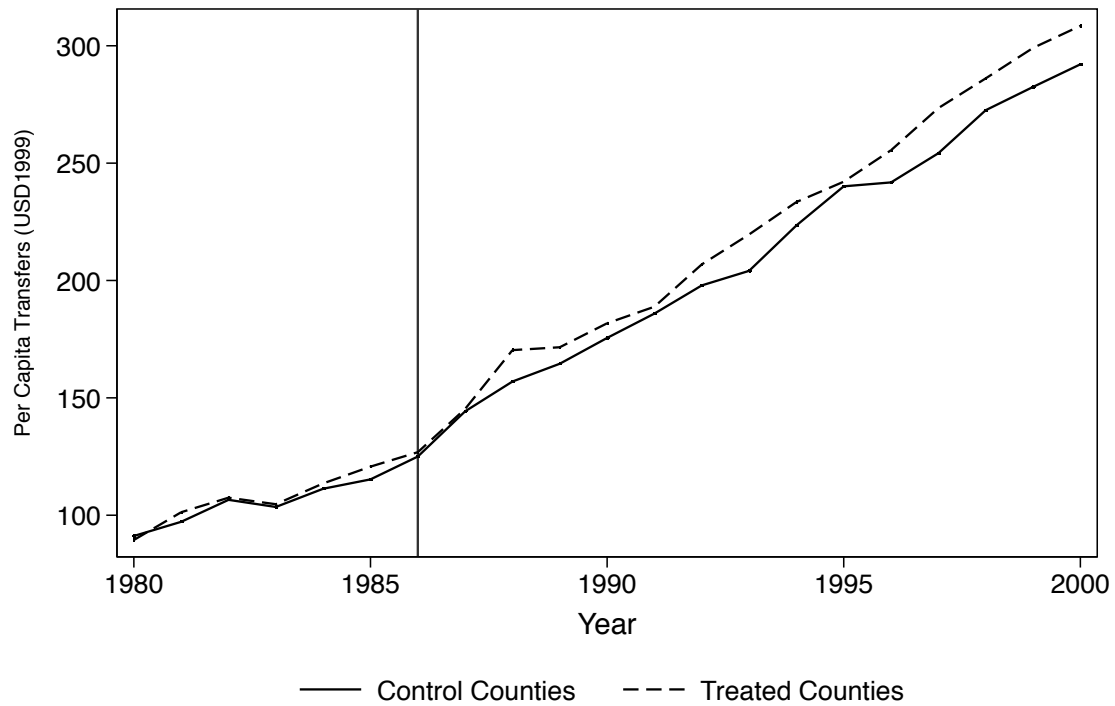
**Figure A.2:** Sources of local government revenue

**Note:** This graph plots the share of local government revenue (cities, municipalities and counties aggregated to the county) coming from state transfers and state and federal transfers.



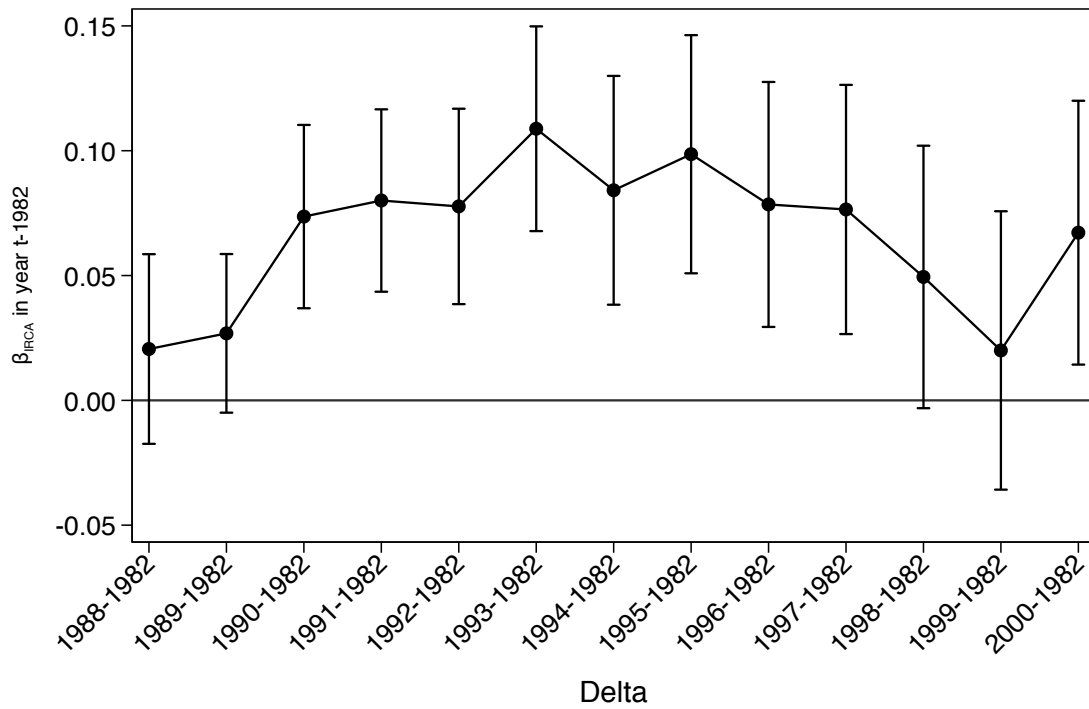
**Figure A.3:** Trends in county socio-economic characteristics

**Note:** This graph compares the evolution of various county characteristics in those counties that never received applications for legal status (control) with those counties that did receive applications for legal status (treated) through the IRCA.



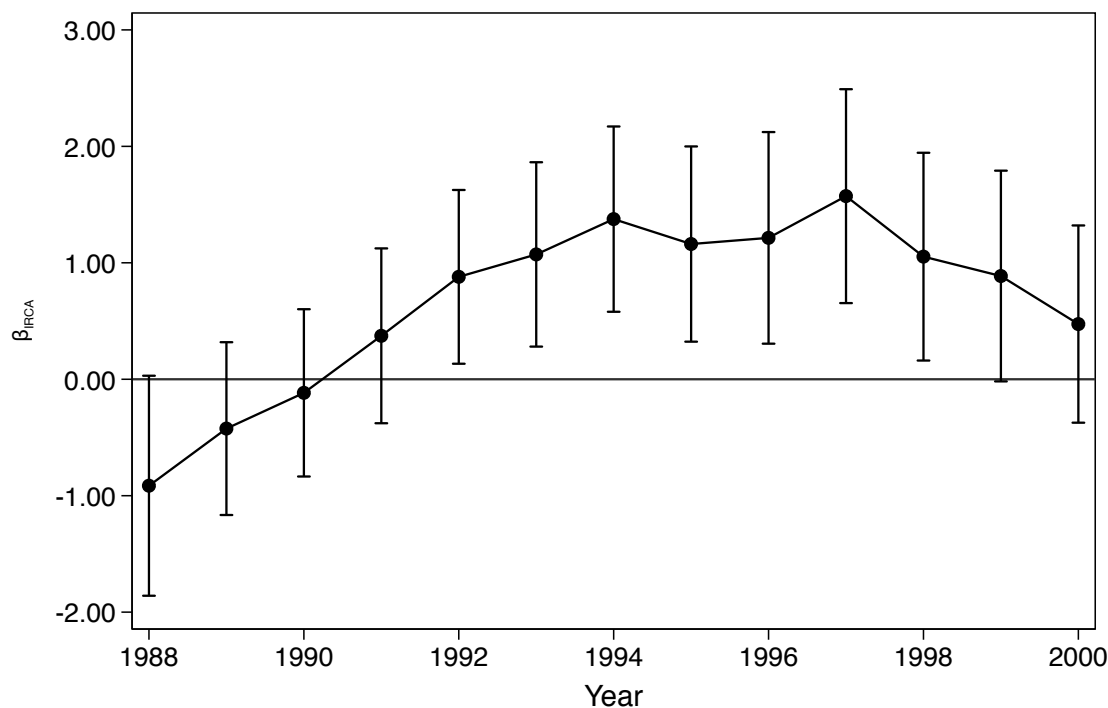
**Figure A.4:** Evolution of inter-governmental revenues in matched sample

**Note:** This graph compares per capita inter-governmental revenues (in USD1999) in those counties that never received applications for legal status (control) with those counties that did receive applications for legal status (treated) through the IRCA in a sample of treated and control counties matched on the basis of propensity scores using the nearest neighbour. The county characteristics on which we base the propensity score matching are county income, population, crime, tax revenue, poverty rate and unemployment in 1980.



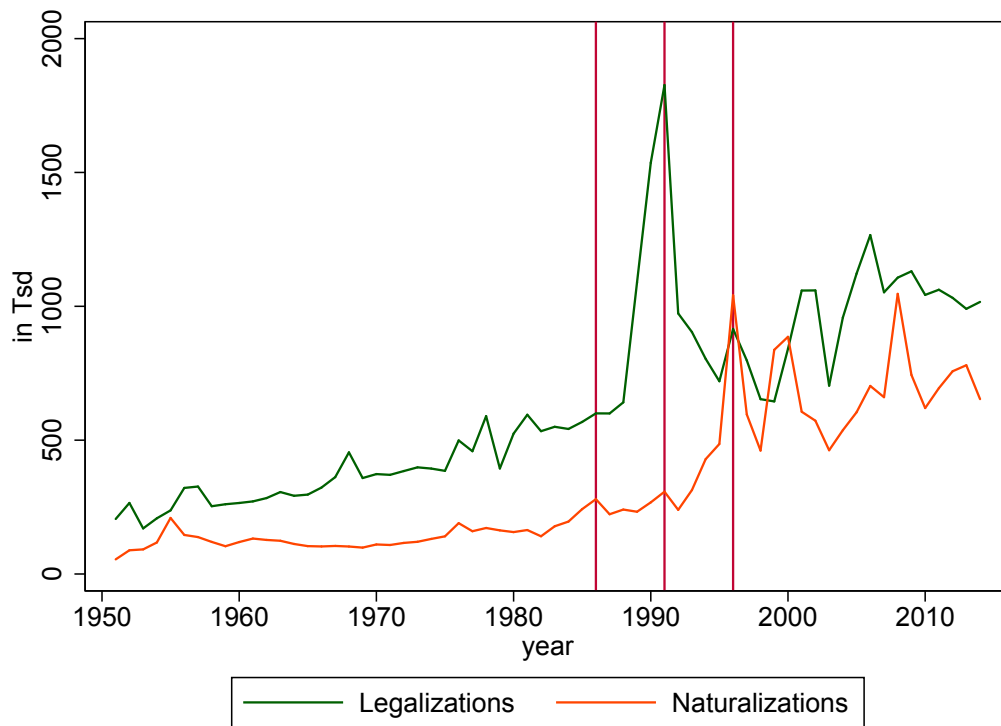
**Figure A.5:** First-difference coefficient estimates

**Note:** This graph plots the coefficients from various first-difference regressions from 1988 to 2000 using 1982 as the base year. The dependent variable is the log of per capita transfers from state to local governments (in 1999 USD) and  $\beta$  is the coefficient on the natural log of the cumulative number of IRCA applicants per 1000 county inhabitants (plus one). Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. County fixed effects and state-year fixed effects are also included in the estimations. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.



**Figure A.6:** Instrumental variables regression coefficients

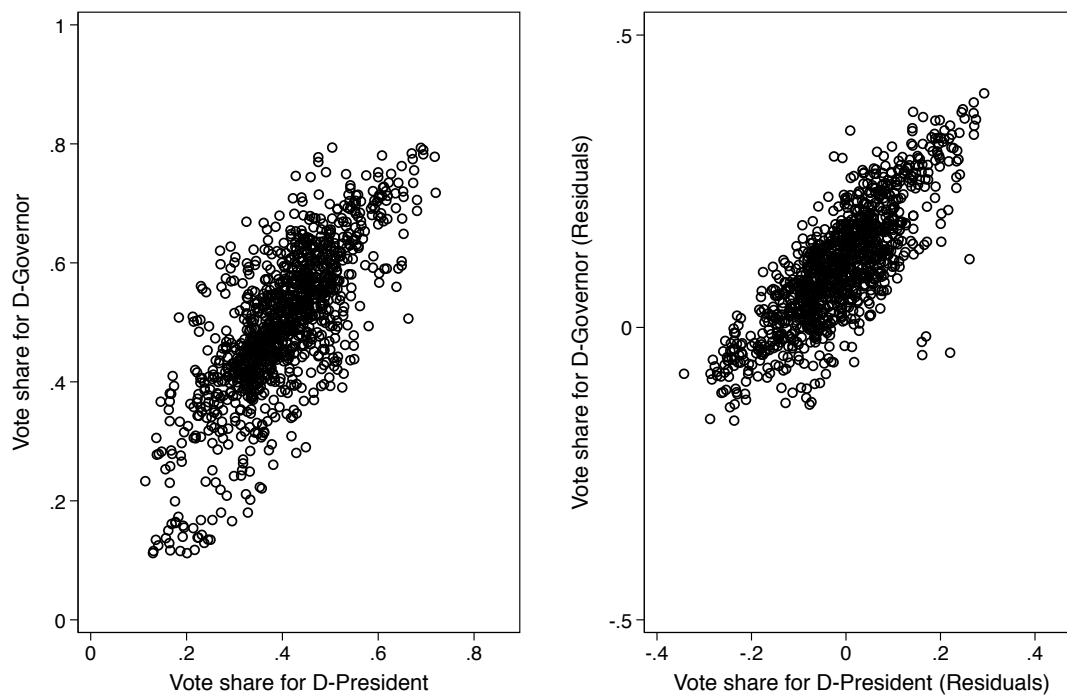
**Note:** This graph plots  $\beta$  from 13 cross section regressions as specified in equation 5, one regression each for the years between 1988 and 2000. For each year, the value of the covariates is differenced from their 1982 value. The dependent variable is the log of per capita transfers from state to county governments (in 1999 USD) and  $\beta$  is the coefficient on the natural log of the cumulative number of IRCA applicants per 1000 county inhabitants (plus one) when it is instrumented by the 1960 share of a county that is foreign-born. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.



**Figure A.7:** Naturalisation and legalisation at the state level

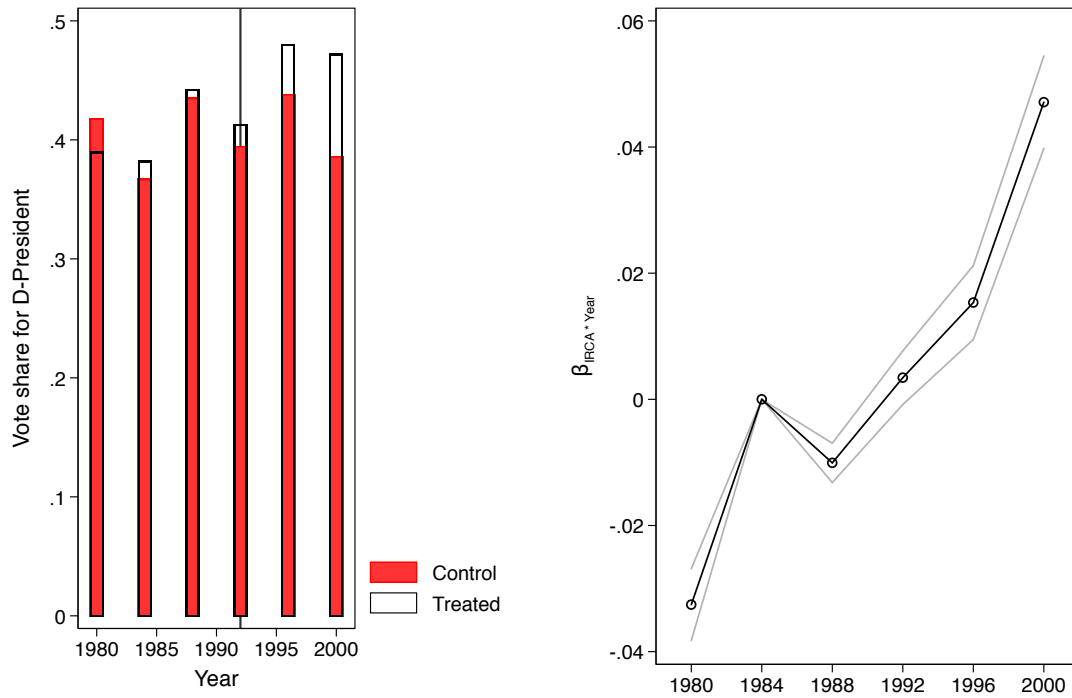
**Note:** This graph plots trends in naturalisations and legalisations at the state level. Lines drawn at 1986 (the passage of the IRCA), 1992 (first cohort of naturalisations arising from the IRCA) and 1996.

**Source:** Immigration and Naturalisation Services, own data.



**Figure A.8:** Presidential and Gubernatorial election results

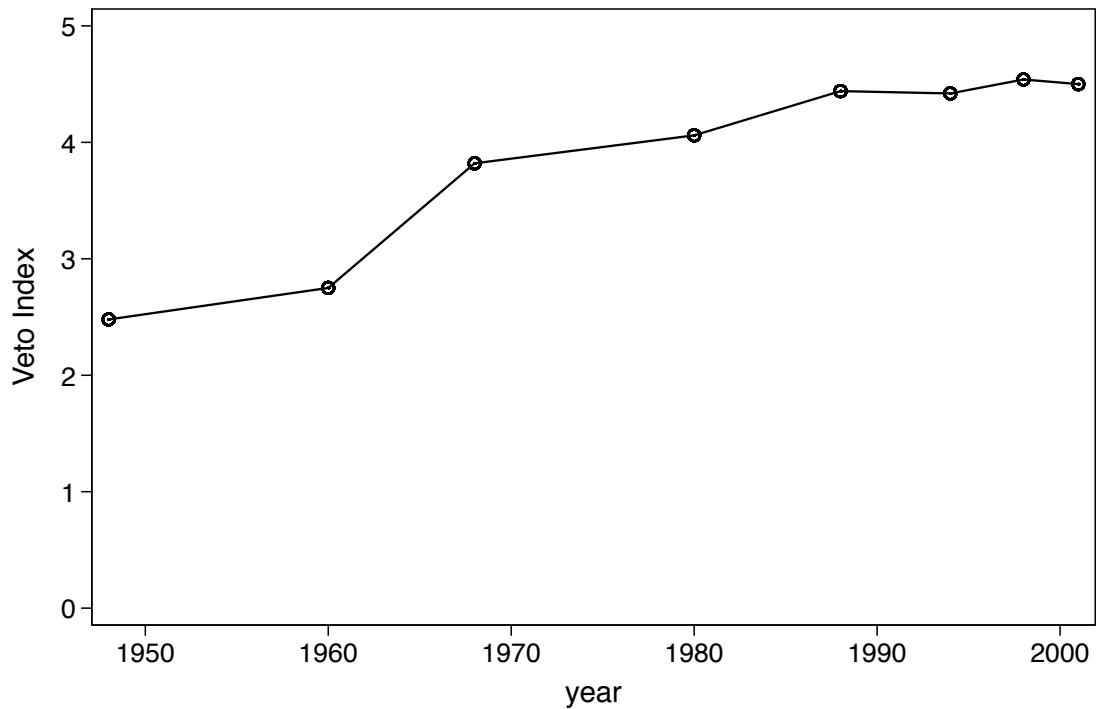
**Note:** These Figures plot the Democratic vote share at the county level in Presidential and Gubernatorial elections beginning in 1992. The scatter on the left plots the raw data while the scatter on the right plots the variables once state-year fixed effects and county fixed effects have been accounted for.



**Figure A.9:** The IRCA and the Democratic vote share

**Note:** The panel on left plots the Democratic vote share at the county level in Presidential elections in counties affected by the IRCA against those not affected by the IRCA. The panel on the right shows coefficients from a regression where Democratic vote share (in Presidential elections) is regressed on an interaction between a treatment indicator and year. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level as well as county fixed effects and state-by-year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.  $N = 12,754$

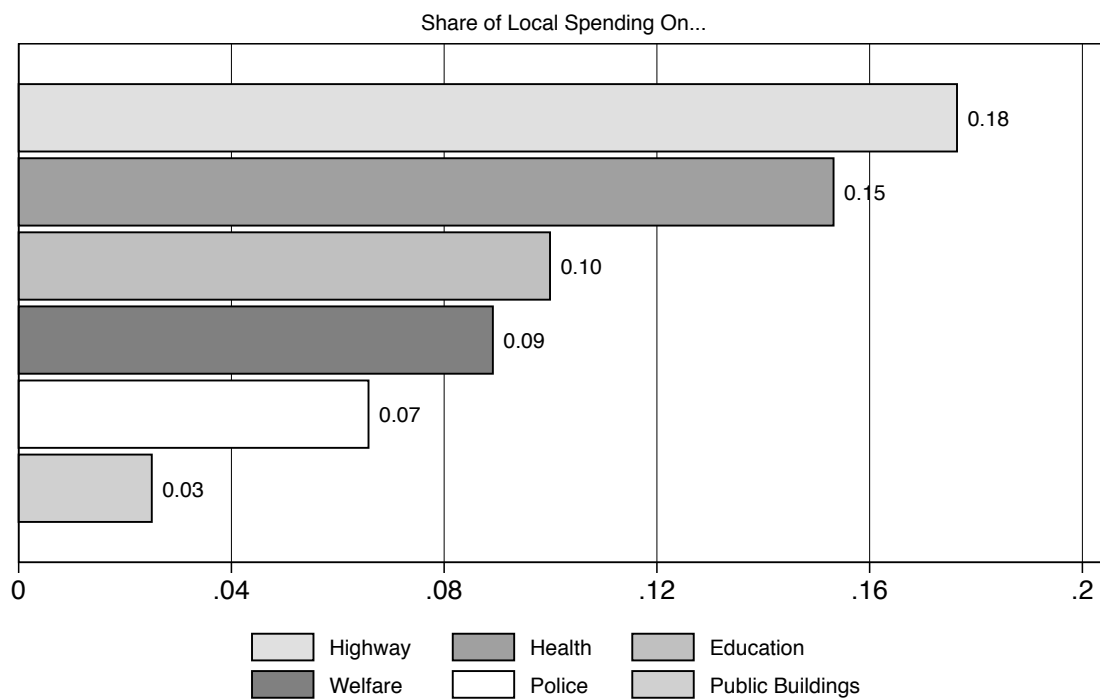




**Figure A.10:** Governor veto power index over time

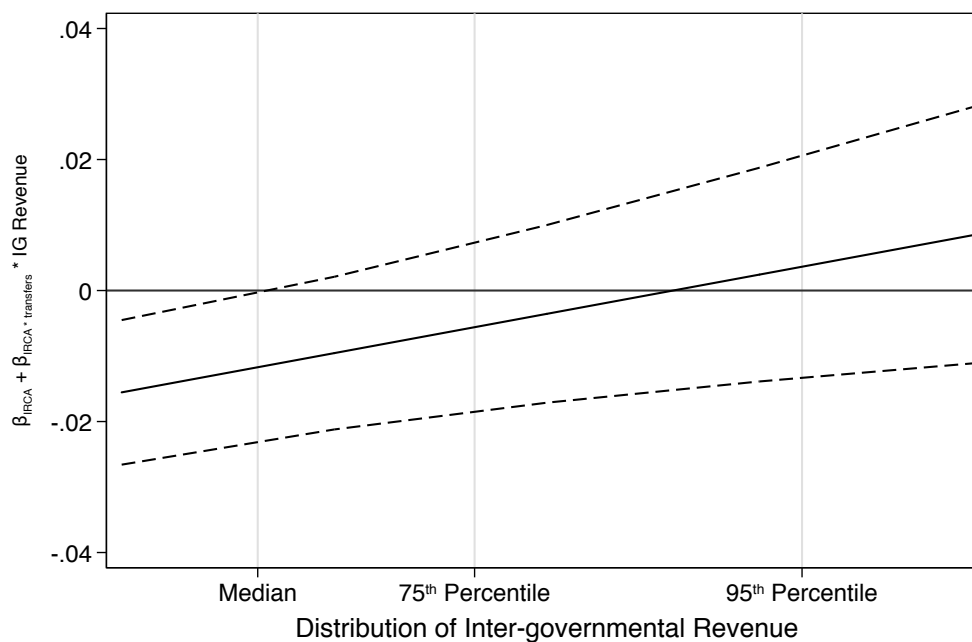
**Note:** This graph plots an index of veto power enjoyed by state governors over time. The index is interpreted as follows: 5 Governor has item veto and a special majority vote of the legislature is needed to override a veto [three-fifths of the legislators elected or two-thirds of the legislators present]; 4.5 Governor has item veto, with a majority of legislators elected needed to override, except for appropriations bills when votes of two-thirds of those elected are needed to override; 4 Governor has item veto with a majority of legislators elected needed to override; 3 Governor has item veto with only a majority of legislators present needed to override; 2 Governor has no item veto, with a special legislative majority needed to override; 1 Governor has no item veto, with only a simple majority needed to override; 0 Governor has no veto of any kind.

**Source:** Thad L. Beyle



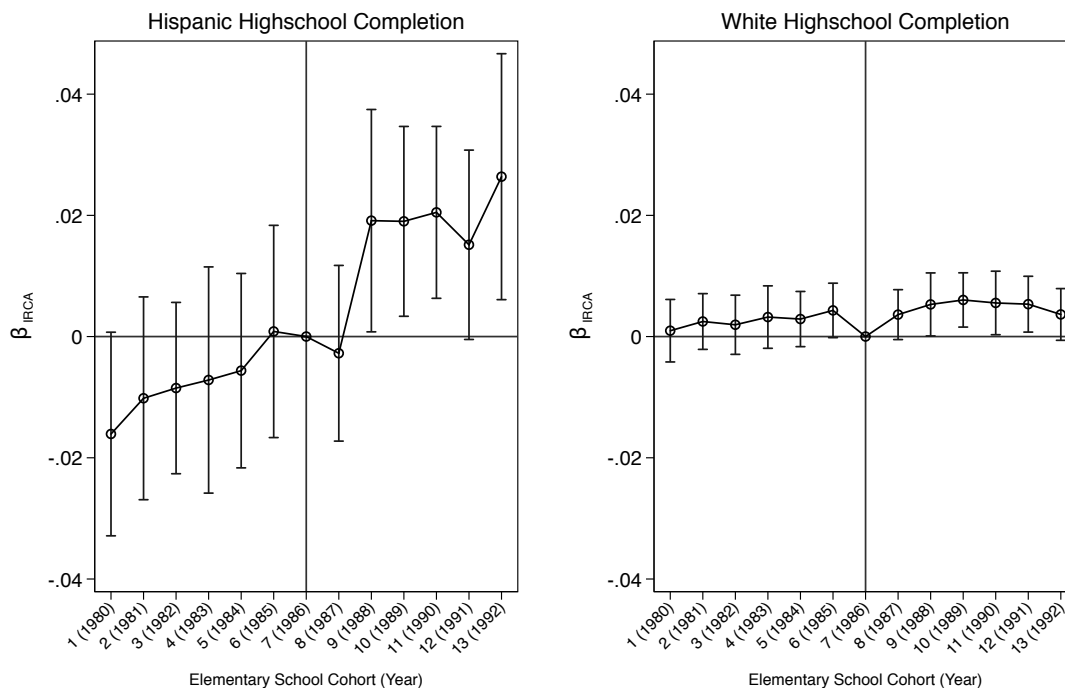
**Figure A.11:** Share of local expenditure on...

**Note:** This graph plots various categories of local government expenditure as a share of total local expenditure.



**Figure A.12:** Marginal effect of the IRCA on the propensity to vote

**Note:** This graph plots the marginal effect of immigrant legalisation on the propensity to vote when immigrant legalisation is interacted with inter-governmental revenue from the state. In other words, it plots  $\frac{\partial \text{vote}}{\partial \text{IRCA}} = \beta_{\text{IRCA}} + \beta_{\text{IRCA} \times \text{transfers}} * \text{transfers}$  along the distribution of inter-governmental revenue.  $\beta_{\text{IRCA}}$  is the coefficient on the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). The regression draws on individual data from the 1996, 1998 and 2000 November Voter Supplement of the CPS. The outcome variable is an indicator that is one if an individual voted in that year's November election and zero otherwise. Control variables include individual race, sex, family income, marital status, education and age as well as year dummies and county population. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.  $N = 41,968$ .



**Figure A.13:** Event study estimates of high school completion on legalisation

**Note:** This graph plots the regression coefficient on legalisation intensity in 1992 when it is interacted with elementary school entry cohort dummies. A person is placed in a elementary school entry cohort depending on the year in which they were six years of age. The outcome variable, taken from the 2010 decennial census, is an indicator that is one if an individual in a given county and middle school entry cohort completed high school and zero otherwise. The regressions include county and cohort fixed effects. The panel on the left plots coefficients when the sample is restricted only to Hispanic individuals whereas the figure on the right estimates the coefficients on a sample of only Caucasian individuals. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. For the Hispanic sample,  $N = 53,580$  whereas for the Caucasian sample  $N = 126,493$ .

### A.3 Additional Tables

**Table A.1:** Congressional Vote Record on the IRCA Bill

	House	Senate
Yes	274 (204-D; 70-R)	63 (34-D; 29-R)
No	132 (33-D; 99-R)	24 (5-D; 8-R)
Abstain	26	13

*Notes:* This table shows how the 99<sup>th</sup> Congress voted for the IRCA Bill on 17 October 1986.

*Source:* Congressional Votes Database accessed at [govtrack.us](http://govtrack.us)

**Table A.2:** Inter-governmental Revenue from State to Local Governments: Categories of Revenue

	Education	Health and Hospitals	Highways	Public Welfare
Includes	State aid for support of local schools; redistribution of federal aid for education; handicapped, special, and vocational education and rehabilitation; student transportation; equalization aid; school health; local community colleges; adult education; school buildings; and property tax relief related strictly to school funding.	State aid for local health programmes; maternal and child health; alcohol, drug abuse, and mental health; environmental health; nursing aid; hospital financing (including construction); and hospitalization of patients in local government hospitals.	State aid for construction, improvement, or maintenance of streets, highways, bridges, tunnels, etc.; distribution of state fuel taxes; and aid for debt service on local highway debt.	State aid for public welfare purposes; medical care and related administration under public assistance programmes (including Medicaid) even if received by a public hospital; care in nursing homes not associated with hospitals; federal categorical assistance (e.g., pass through of Aid to Families with Dependent Children, or AFDC); and administration of local welfare programmes.
Excludes	State grants for libraries; state expenditures on behalf of local schools for textbooks, buses, school buildings, etc.; and value of donated food commodities (non-revenue).	State aid for medical care under public assistance programmes such as Medicaid.	State grants for urban mass transit	

*Notes:* This table explains for what purposes inter-governmental revenue from state to local governments (counties, cities, municipalities aggregated to the county) is used for. We only observe these revenues in aggregate at the county level and do not observe the categories. This information is simply informative to give the reader an idea of the sorts of things a state governor can and cannot support with state-to-county transfers.

*Source:* Information taken from The Census Government Finance and Employment Classification Manual which can be accessed at: <https://www.census.gov/govs/www/classrevdef.html>

**Table A.3:** Baseline Results with Alternative Clustering and Inference

	(1)	(2)
	Treatment × Post	Legalisation Intensity
$\hat{\beta}$	0.0709	0.0610
<i>p</i> -values:		
A. Analytical values (clustered at the state level)	0.0325	0.0149
B. Wild Bootstrap values (clustered at the state level)	0.0521	0.0390
Observations	46,820	46,820
Number of States	46	46

*Notes:* This table presents the baseline estimates (Column 1 of Table 2) clustering the standard errors at the state level. *p*-values are derived both analytically, using Stata's conventional `vce(cluster state)` command as well as through Wild cluster bootstrapping generated using Roodman et al. (2018) `boottest` command.

**Table A.4:** Baseline Results using Linear Legalisations

	Log of Inter-governmental Revenue (per capita)				
	(1) Baseline	(2) Drop Top 5	(3) Pop < 409,490	(4) Matching	(5) Linear Trends
Legalisations per 1,000 capita	0.00551*** (0.00149)	0.0129*** (0.00332)	0.00370*** (0.00140)	0.00557*** (0.00164)	0.00412*** (0.00158)
Control Variables	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	No	Yes
Linear Year Trends	No	No	No	No	Yes
Observations	46,820	43,952	45,132	12,042	46,820
Number of Counties	2,686	2,526	2,612	604	2,686

*Notes:* This table replicates the baseline estimation reported in Panel B of Table 2 but using the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants as the key independent variable without a log transformation. The dependent variable is the log of per capita transfers from state to local governments aggregated to the county in 1999 USD. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$



Section 204 of the IRCA outlines the details associated with the State Legalisation Impact Assistance Grants (SLIAG)—a \$1B per year federal funding programme for four years which could be spent over seven years until 1994. SLIAG was designed to compensate states for the extra costs they would incur as a result of the legalisation programme of the IRCA. Specifically, SLIAG funds were intended to assist states to defray expenses in the areas of public health, public assistance and education (Liu (1991); DHHS (December 1991)). It is unlikely that the SLIAG funds are confounding our results for the simple reason that SLIAG was administered through an entirely separate institutional set-up and is not part of the inter-governmental revenue outcome variable that we exploit.<sup>39</sup> Nevertheless, we obtain the amount of funding states received from SLIAG and deduct it from our main outcome variable to create an ‘inter-governmental revenue net of SLIAG’ variable. Results are shown in Table A.5 and confirm that the resource allocation to affected counties is not being confounded by SLIAG funds.

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<sup>39</sup>As part of the IRCA, The federal government instituted the Single Point of Contact (SPOC) system, whereby every state designated its own SPOC so as to create “state-level lead implementation agencies to manage the [SLIAG] programme according to the unique needs and arrangements of the individuals states” Liu (1991). At the federal level, it was the Department of Health and Human Services that received applications for and disbursed the SLIAG funds but SLIAG required that SPOCs coordinate directly with state and local public health, public assistance and education organisations to receive the funds Liu (1991).

**Table A.5:** Transfers on IRCA Legalizations net of SLIAG Funds

	(1) Net Transfers PC
Log legalisations	0.398*** (0.0807)
Control Variables	Yes
Year Fixed Effects	Yes
County Fixed Effects	Yes
State-Year Fixed Effects	Yes
Observations	6,490
Number of Counties	2238

*Notes:* The dependent variable is the log of per capita transfers from state to county governments in 1999 USD net of SLIAG funds received from the federal government. Log legalisations is the log of the cumulative number of IRCA legalised migrants in a given county in a given year per 1000 county inhabitants (plus one). SLIAG was made available until 1991 and so our net transfers variable is defined only until that period which explains the smaller number of county-year observations. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table A.6:** Democratic Vote Share on IRCA Legalisations

	Democratic Vote Share		
	(1) All States	(2) D State	(3) R State
Log legalisations <sub><i>t-5</i></sub>	0.0204*** (0.00175)	0.0223*** (0.00333)	0.0188*** (0.00206)
Control Variables	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
Observations	7,789	4,203	3,586
Number of Counties	2,141	1,127	1,014

*Notes:* The dependent variable is the Democratic vote share, at the county level, in Presidential elections. Log legalisations<sub>*t-5*</sub> is a five year lag of the log of the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants (plus one). In Columns 2 and 3 we split the sample and look at the relationship only in Democratic (Column 2) or Republican (Column 3) states defined by the party of the Governor in the year 1990. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table A.7:** Legalisation and Dynamics of the 99<sup>th</sup> Congress

	Log of Inter-governmental Revenue (per capita)	
	(1)	(2)
Log legalisations	0.0442** (0.0221)	0.0444* (0.0248)
Log legalisations × Majority D-Senators in State	0.0201 (0.0275)	
Log legalisations × Majority D-Members in State		0.0189 (0.0293)
Control Variables	Yes	Yes
County Fixed Effects	Yes	Yes
State-Year Fixed Effects	Yes	Yes
Observations	46,820	46,820
Number of Counties	2,686	2,686

*Notes:* The dependent variable is the log of per capita transfers from state to county governments in 1999 USD. **Log legalisations** is the log of the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants (plus one). **Majority D-Senators in State** is an indicator that is 1 if both senators of a given state are Democrats and 0 otherwise. **Majority D-Members in State** is defined similarly: it is 1 if the majority of congress members from a given state were Democrats and 0 otherwise. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table A.8:** Legalisation and Term limits

	Log of IGR (per capita)
	(1) Incentive
Log legalisations	0.254 (0.197)
Log legalisations × Electoral Incentive	0.0923*** (0.0241)
Control Variables	Yes
County Fixed Effects	Yes
State-Year Fixed Effects	Yes
Observations	12,134
Number of Counties	2,384

*Notes:* The dependent variable is the log of per capita transfers from state to county governments in 1999 USD. *Log legalisations* is the log of the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants (plus one). *Electoral Incentive* is an indicator that is 1 if a governor is not a lame duck in the period between 1989 and 1994 and zero otherwise. The baseline effect of *Electoral Incentive* is captured by state-year fixed effects and is thus unable to be estimated. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table A.9:** Local Spending and IRCA Legalisations

	Log of Per Capita Local Expenditure				
	(1) Total	(2) Health	(3) Education	(4) Welfare	(5) Highway
Log of Transfers	0.254*** (0.0184)	0.285*** (0.0228)	0.0435*** (0.0113)	0.205*** (0.0199)	0.245*** (0.0193)
Control Variables	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	36,869	36,869	36,869	36,869	36,869
Number of Counties	2,638	2,638	2,638	2,638	2,638
<i>p</i> -values:					
A. Unadjusted	9.8e-42	8.0e-35	.00011	2.0e-24	6.8e-36
B. Westfall & Young Adjusted	0	0	0	0	0

*Notes:* This table presents regression results using various categories of per capita local government expenditure as the outcome variable. *Log of Transfers* is the per capita inter-governmental revenue from the state to local governments aggregated to the county in 1999 USD. The adjusted *p*-values in the last row are adjusted to take into account potential multiple hypothesis testing using Westfall and Young (1993) family-wise error rate procedure. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table A.10:** The IRCA, Citizenship and Voter Turnout using CPS

	(1) Naturalised	(2) Voted in Nov	(3) Voted in Nov
Log legalisations	0.0221*** (0.0046)	-0.0060 (0.0051)	-0.0770*** (0.0207)
Log of Transfers			-0.0086 (0.0085)
Log Legalisation × Log Transfers			0.0126*** (0.0036)
Individual Controls	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	50,206	46,210	41,968

*Notes:* This table presents regression results using individual data from the 1996, 1998 and 2000 November Voter Supplement of the CPS. Log legalisations is the log of the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants (plus one). Log Transfers is the per capita inter-governmental revenue from the state government to the county in 1999 USD. Column 1 has an outcome variable that is 1 if an individual is a naturalised citizen and 0 if (s)he is a native citizen. Columns 2 and 3 have as outcome variables indicators that are 1 if an individual voted in that year's November election and 0 if (s)he did not. The years for which we have data include three major gubernatorial election cycles as well as two Presidential elections but we cannot distinguish which election an individual voted for. Control variables include individual race, sex, family income, marital status, education and age. Standard errors (shown in parentheses) are clustered at the county level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$